Master Thesis

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Taxing Externalities of Individuals' Mobility: Income-related Differences in Response

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Abstract

In the light of recent policy developments in Switzerland, a randomized control trial, which tracked the mobility behavior of about 3,500 individuals, was conducted. Treatments consisted of a purely information-based nudge and a Pigovian tax, charging the marginal external costs of individuals. This thesis focuses on the effect heterogeneity concerning income per person using DiD and quantile regression. The nudge is shown to have no significant effect, while high incomes react most to the Pigovian tax. Additionally, it is found that individuals generally react more, the more external costs they initially have. However, this trend is not true for those with the highest external costs, where both treatments show a high variance in treatment response.

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1 Introduction

The mobility behavior of individuals has stark impacts on their surroundings and the society they live in. These negative external effects might take the form of badly congested cities, greenhouse-gas emissions, or constant noise pollution caused by extensive car use. However, negative external effects are not at all limited to cars. Public transport possesses the same issues to a lesser extent, but even the use of bicycles has negative effects, as bikers are often involved in severe accidents, leading to society having to pay for the medical treatment costs.

Many instruments and policies aim to tackle some of these problematic by-products of our modern mobility behavior. In an extensive randomized control trial (RCT), the feasibility and effect of two such instruments were recently tested by Axhausen et al. (2021). The MOBIS experiment tracked the mobility behavior of around 3,650 individuals in Switzerland for eight weeks, using their smartphone location, and calculated the external effects for each trip and mode. The RCT included two treatments which were introduced after four weeks. To one part of the participants, the monetized sum of their external effects was shown (nudging), while to another part, the same information was shown, but these participants then also had to "pay"[1] for their external effects.

In the light of such instruments, it is vital to anticipate how different sub-groups of the population would be affected by such policies. Of particular concern is the asymmetric effect on different income groups, which often is one of the central points in any public debate of environmental policies. Therefore, this thesis aims at shedding light into the heterogeneity of response of different income groups to the introduction of a nudge and pricing scheme based on the data generated by the MOBIS experiment. The analysis of the treatment effect heterogeneity already included in Axhausen et al. (2021) is extended by focusing on a newly generated variable of equivalized income per person (opposed to household income). As in the main report on the RCT, a difference-in-differences approach is chosen to estimate the causal average treatment effects. Additionally, this thesis extends the analysis beyond average effects and considers the treatment effects at different quantiles of the outcome distribution. This is done using quantile regressions and the recently published estimation approach by Callaway et al. (2018).

In Switzerland, introducing a mobility pricing policy, similar to the MOBIS experiment, has experienced ever more interest over the past decade (Swiss Federal Council, 2016). On the 3rd of February 2021, the Swiss Federal Council started the consultation for a law that would establish a legal framework for pilot studies of mobility pricing schemes in interested cantons (Federal Roads Office - ASTRA, 2021). With this development, the findings of this thesis could show to be very relevant to the policy design in the coming years.

[1] The participants received a virtual budget, based on their mobility behavior prior to the treatment, from which the monetized external costs were deducted.

2 Background

2.1 Transport Externalities

Externalities are generally defined as the external effects of the economic activity of an agent on society that is not taken into consideration by the agent, i.e., do not enter his utility function (Verhoef, 2000). These external effects can be positive or negative and are denoted in monetary values in economic models, thus as external costs or benefits. Externalities can be assessed in terms of total, average, or marginal external costs or benefits. This thesis focuses on marginal external costs (benefits), i.e., the additional external costs caused by a unit increase in the problematic (or beneficial) activity.

Delft (2019), the official handbook of the European Union on transport externalities, distinguishes between multiple types of externalities caused by transportation and provides estimates for the external costs. The general external transportation costs are subdivided into costs related to accidents, air pollution, climate change, noise, congestion, well-to-tank emissions, habitat damage, and other external costs. These cost categories are then further dissected into smaller components and their extend of external (opposed to internal) costs discussed. For a complete presentation of these categories the interested reader is referred to Delft (2019).

2.2 Theoretical Basis of the Treatments

After introducing the concept of externalities, the following gives a short introduction on the theoretical background of the two treatments applied in the MOBIS experiment.

Generally, the competitive market outcome will not be Pareto-optimal when the external effects are not internalized in the decisions of economic agents (Verhoef, 2000). Economists have proposed many instruments to correct this market failure. One of them, which is the most relevant one for this thesis, is to impose a tax (subsidy) on the good producing the negative (positive) externality. Pigou (1920) was arguably the first to argue that in a first-best world with non-distortive taxation, this tax (subsidy) should equal the marginal external damage (benefit) of the activity. This means that in the example of mobility externalities, one should levy a tax on transportation activity in the size of its overall marginal external costs and benefits. Thus, in an ideal, first-best world, one would charge individuals the exact amount of their marginal external costs, which would lead them to adjust their consumption decision and thus restore the socially optimal level of mobility (Verhoef, 2000). However, although theoretically optimal, to implement such a first-best tax, one would have to know the marginal external effects of all traffic participants at any given time and place. Additionally, the charging mechanism would have to be optimal, meaning that exactly these costs can be charged of the right individual (Verhoef, 2000). Despite the difficulty of implementing such a Pigovian tax, Verhoef (2000) argues that second-best policies should still aim at achieving the same incentives and not ignore the first-best result because of its theoretical nature. As will be seen in Section 4, the MOBIS experiment followed Verhoef (2000)s argument and aimed at achieving the first-best tax with one of the treatments.

As an alternative to the classical “hard” approach of internalizing the external effects using a tax scheme, there has been rising interest in so-called “soft” transport policy measures (Möser and Bamberg, 2008). These approaches generally differentiate themselves from the standard economic approach by trying to “influence individual decision making less by using force and restrictions, but rather by persuasion that is by changing people’s perceptions and motivations” (Möser and Bamberg, 2008). One example of such a “soft” policy are information-based nudges. The term nudge was introduced by Thaler and Sunstein (2008), who define it as “any aspect of the choice architecture that alters people’s behavior in a predictable way without forbidding any options or significantly changing their economic incentives” on page 6 of their book. In the context of transport externalities, a nudge could, for example, consist of informing individuals about the external costs their mobility behavior is causing, as was done in the MOBIS experiment.

3 Literature Review

This section gives a short overview of the existing literature relevant to this thesis. Axhausen et al. (2021) have already performed a thorough literature review of studies that analyze the effects of some forms of mobility pricing and information-based treatments. Therefore, this literature review will only give a brief summary and focus on studies that also commented on the effect heterogeneity regarding income.

Some cities and regions have implemented congestion pricing to relieve the chronically crowded city and business centers. The example in London, charges drivers entering a specified area during rush hour and has been analyzed by Leape (2006). In Sweden, similar schemes have been implemented in Stockholm and Gothenburg, which were analyzed by Eliasson et al. (2009), Karlström and Franklin (2009), and Börjesson and Kristoffersson (2018). Singapore also has a long history of measures against congestion, the most recent one charging vehicles upon each passing of a gantry Agarwal and Koo (2016).

Apart from the real world examples above, there have also been some smaller-scale experiments measuring the effects of dynamic mobility pricing. These include experiments in Copenhagen (Nielsen, 2004) and the Netherlands (Ben-Elia and Ettema, 2011), which aimed at breaking the congestion peaks during rush hour. In another recent study, Martin and Thornton (2018) analyzed a field experiment in Melbourne, where 1400 vehicles were tracked using GPS devices for nine months. Randomly treated participants were exposed to various pricing schemes. Besides the overall treatment effects, the study finds that low-income drivers respond most to the pricing scheme implemented. They also find that higher incomes contribute more to congestion externalities and conclude, that low-incomes would benefit from a more individualized road pricing based on a pay-as-you-use principle.

Axhausen et al. (2021) highlight that all studies mentioned so far did not involve a control group and are thus relying on the assumption that no external influences were obscuring the treatment analysis. However, an RCT was recently published by Rosenfield et al. (2020) and involved incentivizing employees to commute by public transport.

Information-based policies can take many forms. The already mentioned study by Möser and Bamberg (2008) summarizes and analyzes 141 studies conducted in this area. Apart from that, some studies have implemented information-based treatments in an RCT (Kristal and Whillans, 2020) or based on mobile applications (Bothos et al., 2014; Carreras et al., 2012; Cellina et al., 2016). Few found significant effects and especially the latter have issues with small sample sizes.

This thesis differentiates itself from the existing literature in multiple ways. First, the MOBIS experiment, in contrast to almost all studies mentioned above, involves a control group. Second, this thesis, in particular, differs from the literature by considering the treatment effects at different quantiles of the outcome distribution, i.e., quantile treatment effects. To the best knowledge of the author, this method has not been applied to this field before.

4 Data

The final report of Axhausen et al. (2021) undoubtedly is the most important piece of literature to this thesis, as its authors conducted the MOBIS experiment. This section, therefore, starts with a general description of the MOBIS study. Thereafter, the changes made to the data and the resulting data set are presented.

4.1 MOBIS Experiment

The Mobility behavior in Switzerland (MOBIS) study is a joint research effort of the University of Basel, the Swiss Federal Institute of Technology, and the Zurich University of Applied Sciences. One main aim of the experiment was to analyze the treatment effects of a Pigovian tax (first-best pricing) and a nudge on individuals' mobility behavior in Switzerland. To this end, a field experiment was conducted from September 2019 to January 2020, tracking the mobility behavior of 3,656 participants, accompanied by questionnaires before and after the tracking period. Later, the study was continued to assess the impact of the COVID-19 crisis on individual mobility behavior. The following summary of the study setup is solely based on the description in Axhausen et al. (2021).

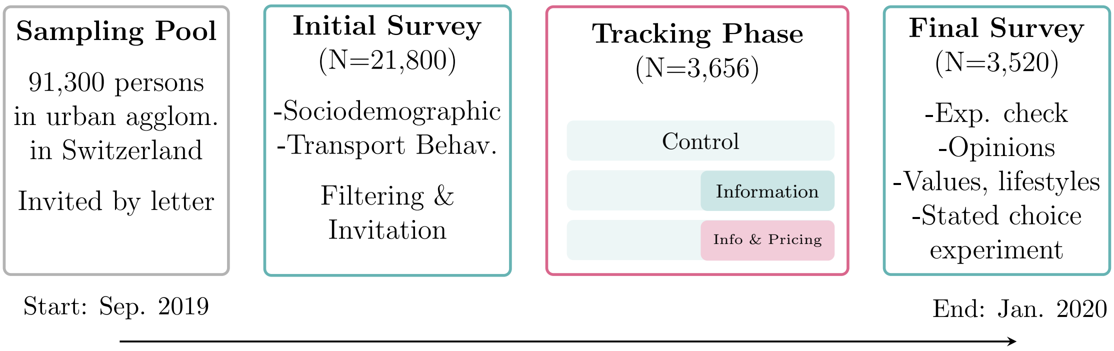
The MOBIS study initially contacted 90,909 individuals aged between 18 and 56 years in 2018 with an invitation letter. Importantly, only individuals in French- or German-speaking cantons living in an agglomeration area were contacted. They had to fill out an initial survey that assessed their socio-demographic and economic characteristics and included questions about their general mobility behavior and views on transport-related topics. This survey was then used to select individuals who qualified for the main experiment. Subjects were required to travel by car at least two workdays per week and possessing a smartphone compatible with the tracking app. Especially the requirement of frequent car use was very restrictive as only 54% of the respondents satisfied this condition (Axhausen et al., 2021). Lastly, individuals should be able to walk 200 meters without assistance and not work as a professional driver to ensure that they have free mode choice.

Eligible individuals were promised an incentive of 100 CHF upon completion of the tracking part which took eight weeks in total. In the first four weeks, all participants received weekly summaries of their travel behavior (via email). This included simple information on which mode of transport they had used for what distance in total within the last week. Tracking was done with the mobile app *Catch-My-Day*[2] which imputed the mode of transport and the purpose of the following activities. Participants could confirm and correct trips and activities recorded by the app. After 28 days the participants were divided into control and two treatment groups. One part was henceforth informed about the external costs of their mobility behavior (*information treatment*). Individuals receiving the *pricing treatment* also received this information, but additionally were assigned a virtual budget which amounted to 120% of their total external costs of the first four weeks of the study. They were informed, that they would receive the remainder of their budget at the end of the study in addition

[2]Developed by Motiontag: `https://motion-tag.com`

to the initial 100 CHF[3]. For the remaining individuals, nothing changed so their mobility behavior in the post-treatment phase could be used as control. Upon completing the eight weeks of tracking, the participants were required to fill out a final survey. Figure 4.1 gives a graphical overview of the study setup.

Figure 4.1: Overview of the MOBIS study. Based on Axhausen et al. (2021)



Three different types of externalities were considered in the MOBIS experiment and imputed from the recorded travel behavior - *congestion*, *climate*, and *health* related external costs and benefits. Importantly, marginal external costs were considered. For car travel the external costs were computed by mapping the travel recordings to the Swiss road network and combining them with official emission factors (HBEFA) and the agent based model MATSim[4]. Congestion costs were measured in seconds of caused delay, climate externalities consisted of CO2 emissions and health externalities of the effects caused by nitrogen oxides (NO_x) and particular matter ($PM_1$0). To reflect crowded coaches, congestion externalities were also included for public transport at specific times and places. Overall, health costs included costs borne by society due to pollution and accidents (healthcare costs), as well as noise. Active modes were the only ones to exhibit external benefits by reducing the healthcare costs. However, for bicycles this was outweighed by the elevated risk of severe accidents. The resulting external costs per mode are shown in Table 4.1[5]. For a complete description of how the externalities were imputed see Axhausen et al. (2021).

Table 4.1: Average external costs - CHF/Km

	CO_2	Congestion	Health	Total Exter. Cost
Car	0.326	0.330	1.014	1.670
Bus	0.040	0.033	0.148	0.221
Tram	0.000	0.048	0.034	0.082
Train	0.002	0.322	0.296	0.621
Bike	0.000	0.000	0.296	0.296
Walk	0.000	0.000	-0.069	-0.069

Notes: Reprinted from Axhausen et al. (2021).

[3]Naturally, participants did not have to pay if they exceeded 120% of their pre-treatment externalities.
[4]`https://matsim.org/`
[5]For modes other than cars, usual per kilometer external costs were applied to the recorded trips.

Comparing the externalities considered in the MOBIS study with the categories described by Delft (2019) shows that most transport externalities were considered (exception are well-to-tank and natural habitat damage). This and the fact that marginal external costs were computed on a time and location basis allows the study to get close to the first-best (Pigovian) externality tax described in Section 2. The study's combination of tacking data and sophisticated externality computation can be said to fulfill the requirements of "some 'Big Brother' type of electronic road charges, using very sophisticated technologies that can monitor the actual emissions" as foreseen by Verhoef (2000) twenty years ago. This combination also sets this study apart from the many studies on mobility behavior that relied on travel diaries or mere questionnaires on mobility behavior Axhausen et al. (2021).

4.2 Data Description

In this section, the data set generated by the MOBIS experiment is presented in more detail. This serves as the basis for choosing the estimation method and highlighting some special properties of the data.

First of all, as this thesis is concerned with analyzing the different income levels, it is crucial to have information on the income level of all individuals included in the analysis. A participant's income was assessed on the household level in the introductory survey, right at the beginning of the MOBIS study. Household income was thereby divided into five groups. Unfortunately, 348 participants were not willing to state their income. In an attempt to reduce the loss of observations, a unique feature of the MOBIS study was exploited. Coincidentally, the world was hit by the corona pandemic right after the MOBIS experiment had concluded. Therefore, the research team decided to start a follow-up study to analyze the change in mobility due to the pandemic. In the course of this new research project, individuals were asked again about their household income level. Using this information, the missing income of 36 individuals was replaced, resulting in a sample size of 3,344 individuals. However, some individuals had very few observations in the post-period phase. These could spoil the regression results, as they are based on very few and possibly outlier observations. The data was consequently additionally restricted to individuals with at least five observations in both the first and second half of the experiment.

The final data set used in this thesis consists of 1,177,363 individual trip legs[6], which are aggregated into 152,429 travel days by 3,235 individuals. As mentioned in Section 4.1, individuals were being tracked over eight weeks between September 2019 and January 2020. In Figure 4.2, the distribution of the recorded travel days over this period is displayed. It shows that most trips were recorded between mid-September to the end of December. Additionally, Figure 4.3 displays the number of travel days recorded for each day in the study (1,...,56). Note that the 29th day was dropped for every individual to reduce the probability that treated individuals had not yet opened the email which delivered the treatment. Simi-

[6]Individual Mobility was recorded in trip legs, which divide a trip into its (possibly) many stages. A work trip could for example start with a walk to the bus from which the individual then would change (walking) to the train. After a short walk from the train station to the office this trip would be over and recorded as five independent trip legs with one unique mode, each being assigned their respective externalities.

larly, the first days were not considered, as individuals were expected to need some time to familiarize themselves with the tracking application. In Figure 4.3, one can also see that observations slightly reduced towards the end of the tracking period. All three groups showed similar developments over the course of the study, and no signs of attrition are apparent in this graph.

Figure 4.2: Observations per date

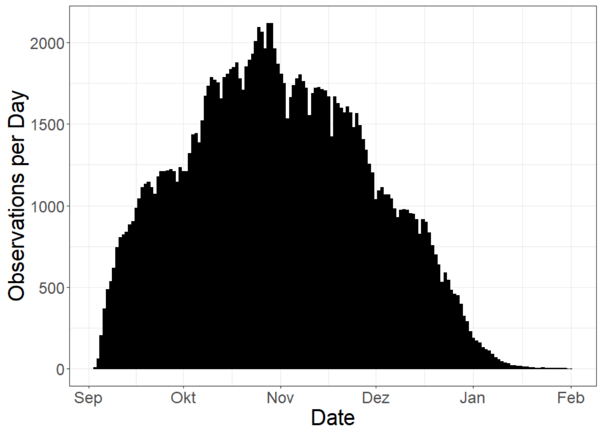
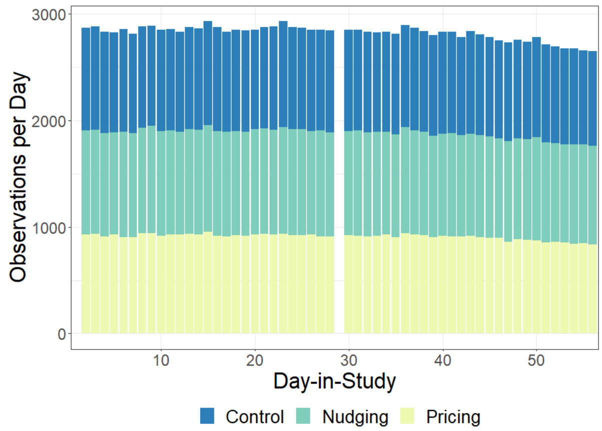


Figure 4.3: Observations per study-day



The two figures above also show that treatment status was assigned at the same point within the study (after 28 days). However, this time point could fall on different dates for the participants, depending on when they started. This will be discussed again in Section 5, which presents the estimation strategies used in this thesis.

For almost every data-based study, the representativeness of the data points collected is vital for the external validity of the findings. To analyze this, Axhausen et al. (2021) compared their data set to a representative Swiss micro-census travel survey that involved about 56,000 individuals (Swiss Federal Statistical Office, 2017). Table 4.2 repeats this exercise and also includes the sub-sample used in this thesis. Due to the different sample restrictions, there are some differences between the MOBIS tracking sample, the sub-sample used in this thesis, and the Swiss population. However, the two samples based on the MOBIS experiment are naturally very similar. Comparing these two to the representative sample of the Swiss population shows the effects of the entry requirements of the MOBIS experiment. Accordingly, no individuals aged less than 18 years and no Italian-speaking individuals are included. Furthermore, the individuals in the MOBIS sample are, on average, more likely to be between 18 and 65 years old, better educated, more likely to be an employee, less likely to be retired, more likely to live in a bigger household, and more likely to be Swiss. Importantly, the observations included in this thesis were also less likely to report an annual household income below 4,000 CHF but more likely to earn more than 8,000 CHF per household. The quite substantial differences in car-use, age, education, employment, and household income challenge the representativeness of the MOBIS data. The consequences of this will be taken up in the concluding Section 8.
Individuals in the MOBIS study were assigned a treatment status after tracking their mobility for four weeks. As in every randomized control trial (RCT), it is worthwhile to check up on

Table 4.2: Comparison of MOBIS data with Swiss Population

		Study		
Category	Level	MOBIS	Thesis Subsample	Mikrozensus
Access to car	Yes	87.7	88.6	75.8
	Sometimes	11.1	10.4	18.1
	No	1.2	1.0	6.2
Age	Under 18	0.0	0.0	13.2
	[18, 25]	19.3	17.6	9.0
	(25, 35]	17.8	18.3	14.2
	(35, 45]	22.3	23.0	15.4
	(45, 55]	22.8	22.9	16.7
	(55, 65]	16.6	17	12.9
	66 and older	1.2	1.3	18.5
Education	Mandatory	6.6	6.8	19.3
	Secondary	48.5	48.3	49.5
	Higher	44.9	44.9	31.2
Employment	Employed	71.3	73.4	48.2
	Self-employed	6.3	6.3	7.2
	Apprentice	1.7	1.6	2.6
	Unemployed	3.9	3.5	2.5
	Student	7.9	6.6	3.7
	Retired	3.2	3.3	19.3
	Other	5.6	5.3	16.5
Gender	Female	50.2	49.4	50.7
	Male	49.8	50.6	49.3
Household size	1	11.7	12.1	34.0
	2	30.2	30.8	35.4
	3	21.5	21.5	13.0
	4	27.4	27.0	12.5
	5 or more	9.2	8.6	5.1
Income	4 000 CHF or less	7.4	8.2	17.8
	4 001 - 8 000 CHF	29.6	32.6	32.8
	8 001 - 12 000 CHF	29.0	32.0	17.4
	12 001 - 16 000 CHF	14.6	16	6.8
	More than 16 000 CHF	9.9	11.1	4.5
	Prefer not to say	9.5	0	20.7
Language	German	66.1	67.4	68.4
	French	26.1	25.2	25.3
	Italian	0.0	0.0	6.3
	English	7.8	7.4	0
Nationality	Switzerland	98.1	98.1	75.9
	Other	1.9	1.9	24.1

Notes: Summary statistics shown for the original Mobis Study, the sub-sample used in this thesis and the Swiss travel micro-census for 2015. Adapted from Axhausen et al. (2021).

the success of the treatment randomization. This is usually done in practice by comparing the mean values of pre-treatment variables between the treatment groups, as is done in Table 4.3. In a successfully randomized experiment, none of the differences should be significant at the 5%-level.

Table 4.3: Means per treatment group of pre-treatment variables

	E(X|D=I)	p-value	E(X|D=C)	p-value	E(X|D=P)	p-value
Male	0.510	0.752	0.503	0.891	0.506	0.861
German	0.665	0.739	0.671	0.398	0.688	0.239
French	0.255	0.948	0.256	0.515	0.244	0.557
Age < 30	0.246	0.585	0.236	0.458	0.250	0.837
$30 \leq$ Age ≤ 55	0.556	0.844	0.552	0.687	0.560	0.835
Age > 55	0.199	0.413	0.213	0.196	0.190	0.627
Primary Education	0.054	***0.005	0.085	*0.083	0.065	0.300
Secondary Education	0.487	0.395	0.469	0.255	0.493	0.765
Tertiary Education	0.459	0.568	0.447	0.820	0.442	0.427
Household Size	2.868	0.524	2.900	0.795	2.913	0.373
Income $\leq$ 4,000	0.231	0.956	0.230	0.842	0.227	0.799
4,000 $<$ Income $\leq$ 6,500	0.132	0.195	0.114	0.432	0.125	0.622
Income $>$ 6,500	0.374	0.700	0.366	*0.079	0.403	0.168
Full-time Employed	0.733	0.979	0.732	0.831	0.736	0.851
Owns Bike	0.730	0.984	0.730	*0.089	0.697	*0.092
Owns Car	0.888	0.791	0.885	0.967	0.884	0.761
Regular Public Transport	0.286	0.382	0.269	0.331	0.288	0.911
Regular Car	0.720	0.622	0.729	0.478	0.743	0.229
Congestion†	1.026	0.373	0.995	***0.007	1.099	*0.055
Climate†	0.856	0.677	0.845	0.549	0.861	0.857
Health†	2.540	0.341	2.468	0.276	2.551	0.885
Total Ext.†	4.422	0.349	4.308	0.107	4.512	0.477

Notes: C = Control, P = Pricing, I = Information. P-value-columns indicate the p-value of the difference between the two neighboring columns. † Average pre-treatment external costs per day. Income variable is denoted in CHF per month and is based on equivalized income as described in the next section. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

It can be seen that the randomization was successful with respect to most variables. Nevertheless, the table shows that the control group was significantly more likely to have attended only primary school. The difference to the information treatment group is significant at the 1% significance level. In addition to that, the pricing group caused significantly more average external congestion costs per day in the pre-treatment phase. Here the difference to the control group is again significant at the 1%-level. Some other insignificant (at the 5%-level) differences were detected in the high income-group and the variable indicating bike ownership and highlighted in the table.

The significance level of the differences, especially in the two variables "primary education" and "congestion", is unusual for a successfully randomized experiment. However, there is a caveat to the results in Table 4.3. One can only expect the difference between two individuals to be the same on average for a specific date and a specific day in the experiment. This argument bases on the fact that individuals experience different external influences on different calendar days (e.g., weekend versus workday or regional holidays) and that there are likely effects from participating in the study that vary over time (e.g., learning effect in using the app, fatigue from participation, etc.). Thus, one can only assume the pre-treatment variables to have the same expectation conditional on calendar day and day-in-study. However, on average, one would expect these differences to disappear, as one would expect equally many individuals per treatment group to record at any given day in the study and date. Apparently, this did not fully occur, leading to the observed differences. One reason for this could be the reduced number of observations at the start and end of the experiment period (September and January), as shown in Figure 4.2. This result has a significant role in the choice of the estimation method.

4.3 Equivalence Income

With the main variable of interest in this thesis being the income variable, it makes sense to consider this variable in more detail. Household income was assessed in the introductory survey[7], where individuals could select their estimated household income from five income ranges or refuse to answer. However, household income is an incomplete measure for a household's living standard because it neglects how many individuals have to be sustained with this income in the household Kuhn (2019). Thus, household income should be set in relation to household size. Furthermore, there is consensus in the literature that one should use equivalence scales to adjust for economies of scale within a household. At the base of this argument lies that two persons living in the same household will not need twice the income of one person to reach the same living standard (e.g., because they split electricity and heating costs). In the most simple form, this is done using the following equation

$$\text{Equivalence Income} = \frac{\text{Household Income}}{\text{Household Size}^{\epsilon}} \tag{4.1}$$

where ϵ is a number between 0 (perfect economies of scale) and 1 (no economies of scale) (Figini, 1998). Commonly $\epsilon = 0.5$ is chosen, resulting in household income being divided by the square root of household size. Of course, many other equivalence scales have been proposed and are being used by different countries and organizations, with the most prominent being the modified OECD scale (Dudel et al., 2020). This scale does rely on the household composition and therefore requires information about the age of its members. Unfortunately, the MOBIS survey did not include this information about the participating households. In this thesis, the square root scale approach is applied because it is very widely used, and at least partially consistent with estimates for Germany (Dudel et al., 2020), a country relatively similar to Switzerland. Naturally, the choice of which equivalence scale is used and how one adjusts for economies of scale, has a strong impact on the degree of inequality and poverty measured in the data (Figini, 1998; Kuhn, 2019). With the modified OECD scale representing an elasticity of about $\epsilon = 0.54$ (Dudel et al., 2020), applying the square root approach in this thesis rather overestimates the economies of scale. Therefore, this leads to conservative results when classifying households as low income. 4.4 displays the distribution of household income and household size.

[7] The exact wording of the question was "*What is your approximate total household income per month? Annual income divided by 12.*". The answer options were: 4,000 CHF or less, 4,001-8,000 CHF, 8,001-12,000 CHF, 12,001-16,000 CHF, more than 16,000 CHF, and prefer not to say.

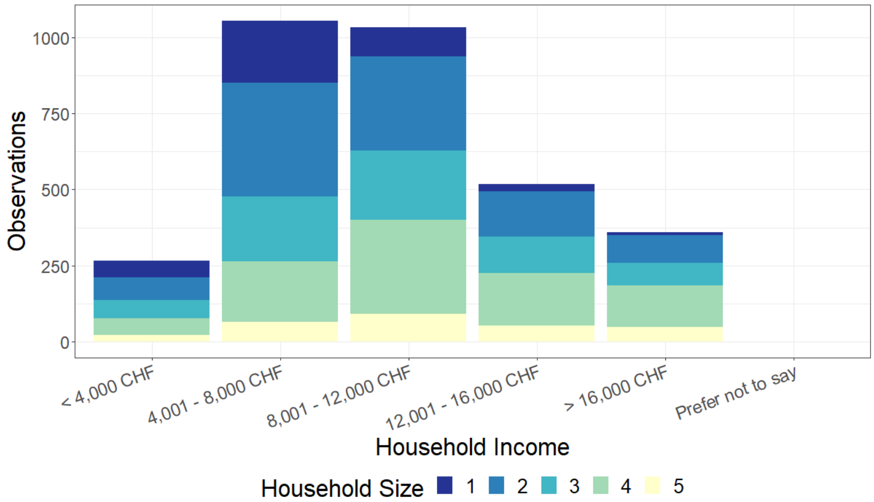
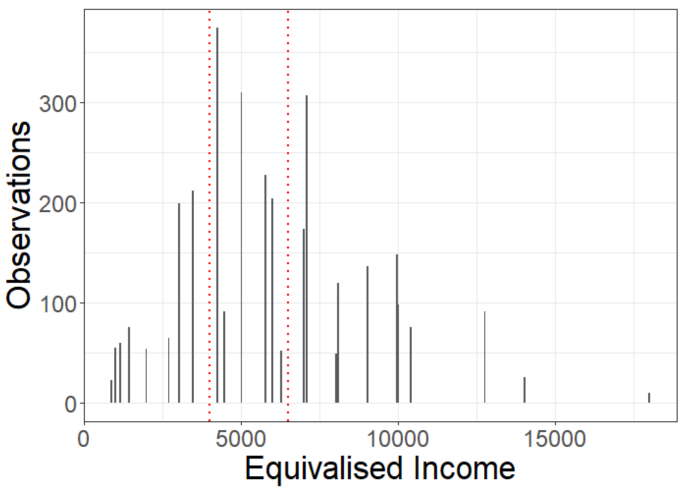


Figure 4.4: Household income and size

As might have been noticed by the attentive reader, the division of household income ranges through the household size is not straightforward, as there is no single income level that could be divided. Kuhn (2019) mentions as a solution to the computation of equivalized income that the middle point of the income bands can be used. This is however not possible for the lowest and highest interval as they lack one bound, thus one has to make an assumption on which representative income level to choose for these groups. Figure 4.5 shows the distribution of the equivalized income, when using the square root approach, 2,000 CHF for the lowest income group and 18,000 CHF for the highest income group. Notably, there are $5 \cdot 5 = 25$ possible combinations of the household income and size variables.

Figure 4.5: Equivalized income



With the equivalized income level of these 25 income groups being highly dependent on the economies of scale (ϵ) parameter, it makes sense to classify the participants into larger income groups. For this division, the equivalized income levels in the MOBIS experiment were compared to the average equivalized incomes in the Swiss population. This allows to infer, if a household is considered to have a low or high income in Switzerland (Swiss Federal Statistical Office, 2021)[8]. Otherwise, one could only make statements with respect to relatively low incomes in the MOBIS sample. Table 4.4 shows the comparison of the equivalized income quartiles between the MOBIS data and the Swiss population.

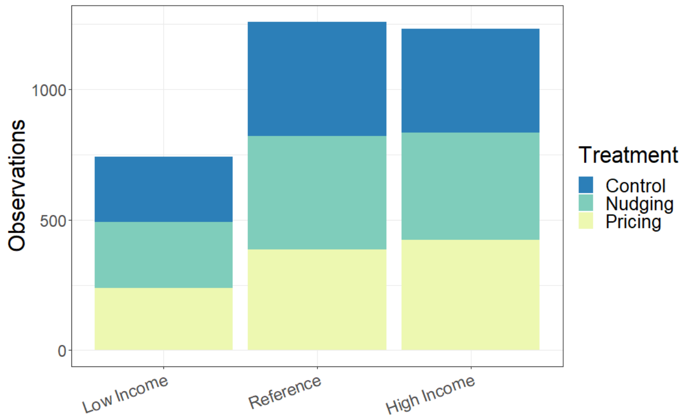
Table 4.4: Quartiles of equivalized monthly income

	MOBIS	Switzerland
1st Quartile	4,255 CHF	3,001 CHF
Median	5,780 CHF	4,173 CHF
3rd Quartile	7,092 CHF	5,664 CHF

Notes: Comparison of equivalized income quartiles in MOBIS and of Swiss population.

The high income of the MOBIS sample poses a problem for the definition of the equivalized income groups, as there would be very few (many) observations in the lower (higher) income groups. Thus, there is a trade-off between correctly categorizing individuals as having a low income, and using enough individuals in each group to ensure statistical validity. Taking this trade-off into account, the cutoff values of 4,000 CHF and 6,500 CHF were chosen. These are represented by the red dotted lines in Figure 4.5. This increased (reduced) the number of low (high) income observations by 212 to 742 (by 484 to 1233) compared to strictly applying the quartiles of the Swiss Federal Statistical Office (2021) as cutoffs. The reference income group contains 1,260 individuals, with this definition of the three groups. The sensitivity of the results on the choice of the cutoffs is checked in Appendix I. Figure 4.6 displays the resulting income groups and the shares per treatment group.

Figure 4.6: Final income groups and treatment status



[8] The Swiss Federal Statistical Office (2021) used the modified OECD scale to determine the equivalized income.

With Axhausen et al. (2021) also using three groups for their analysis of household income, it makes sense to compare how the newly generated equivalized income categories correspond to this original division of the sample. Table 4.5 does this by comparing group membership in the two variables. It can be seen, that using the equivalized income, relatively fewer (more) individuals are considered to have a low (high) income.

Table 4.5: Changes in income group membership

	Equivalized Income			
HH Income	Low	Reference	High	Sum
Low	735	574	0	1,309
Reference	7	634	421	1,062
High	0	52	812	864
Sum	742	1,260	1,233	3,235

Notes: Table showing how many individuals changed income group compared to the definition in Axhausen et al. (2021). Numbers on the diagonal indicate the number of individuals which remained in the same category.

On a final note, there are some possible issues with the method used to assess household income to begin with. First, there might be a selection bias in which individuals are not willing to report their income level at all. Furthermore, respondents usually estimate their income level in survey instead of knowing their exact total income. An important part in this is also the definition of income, which might be understood differently by different people (Kuhn, 2019). Lastly, Angel et al. (2018) found that the income distribution in Austria is less uneven when using survey data compared to register data. They add, that individuals with lower incomes tend to over-report their income, while rich individuals tend to under-report. There are many other reasons why income reported in surveys should be taken with a grain of salt.

5 Methodology

This section presents the methodological concepts used to estimate the results presented in Section 6. First, the more common identification and estimation of the average treatment effect (ATE) with the Difference-in-Differences (DiD) estimator is explained. The second part covers the identification and estimation of quantile treatment effects (QTE). In both parts, the discussion about the validity of the identifying assumption is directly included. Although quite unusual, this structure reduces repetition and allows to focus solely on the analysis in Section 6.

5.1 Average Treatment Effects

Using the potential outcome model notation (Lechner, 2011; Athey and Imbens, 2006; Callaway et al., 2018), assume individuals are observed in two time periods $t \in \{0,1\}$ and can be in two treatment states $d \in \{0,1\}$, where $d = 1$ denotes treated individuals. Then these individuals have potential outcomes denoted by Y_t^I and Y_t^N for individuals in the treated and, respectively untreated state, at time t. Crucially, each individual can only be observed in one treatment state in each time period. The potential treated outcome of the treated in period one, $(Y_1^I \mid D = 1)$, is identified by the observed $(Y_1 \mid D = 1)$, i.e. the outcome of the treated in period 1. $(Y_1^N \mid D = 0)$, $(Y_0^N \mid D = 1)$ and $(Y_0^N \mid D = 0)$ are identified in a similar manner. Given this notation and the fact that it is impossible to observe an individual in both potential outcome states in the same period, causal treatment effect analysis resorts to estimating the average treatment effect on the treated (ATET) of an intervention.

$$ATET_t = \mathbb{E}\left[Y_t^I - Y_t^N \mid D = 1\right] \tag{5.1}$$

In words, the ATET is equal to the difference of the expected potential treated outcome of the treated and the expected potential outcome of the treated, had they not been treated. With $\mathbb{E}\left[Y_t^I \mid D = 1\right] = \mathbb{E}\left[Y_t \mid D = 1\right]$, the fundamental problem is to identify the counterfactual outcome of the treated group in period t, had it not been treated, i.e. $\mathbb{E}\left[Y_t^N \mid D = 1\right]$.

With the treatments being randomly assigned in the MOBIS experiment, one natural approach would be to argue, that $\mathbb{E}\left[Y_t^N \mid D = 1\right]$ is actually identified by the observed control group at time t (Angrist and Pischke, 2008). This bases on the argument, that in a randomized control trial, the only difference between treated and control is that, the treated group receives a treatment and the control group does not. However, in the light of the multi-period panel data structure of the MOBIS data, and the likely date and day-in-study specific effects using a difference-in-differences approach can be said to be more suitable in this situation. As will be presented in the following, this method allows to incorporate all the above and does not depend on randomized treatments.

5.1.1 Difference-in-Differences

At the base of the DiD approach lies the assumption, that the treated group would have followed the same trend as the control group, had it not been treated (Lechner, 2011). The treatment effects are thus calculated as the difference between the differences over time of the two groups. One main feature of this method is, that it does not require the treatment and control group to have the same pre-treatment outcomes, which makes it very flexible to apply to a multitude of research questions. There are, however, some assumption needed to identify the treatment effect (Lechner, 2011).

Identification

$$\text{SUTVA:} \quad Y_t = dY_t^I + (1-d)Y_t^N \quad \forall t \in \{0,1\} \tag{5.2}$$

This first assumption, called the Stable Unit Treatment Value assumption, says that the treatment of the treated group may not have spill-over effects on the control group and vice versa. In other words, the control group may not be affected by the treatment of the treatment group. One possible example for this could be that a person in the control group would benefit from the behavior change of a treated individual, because the latter does not use the same road at the same time anymore, as a response to the received treatment. Although theoretically possible, the scale of the MOBIS study makes these scenarios very unlikely and thus this assumption likely holds.

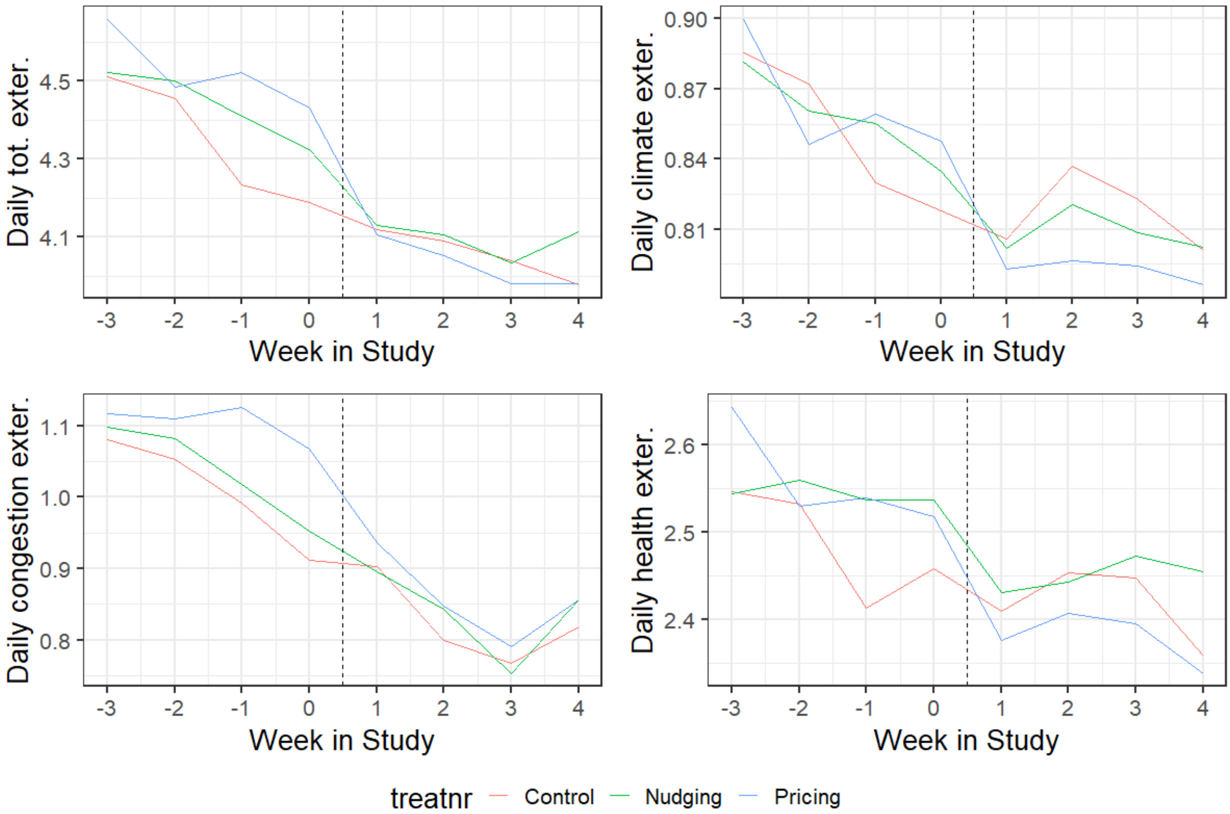
$$\text{No Anticipation:} \quad \mathbb{E}\left[Y_0^I - Y_0^N \mid D=1\right] = \mathbb{E}\left[Y_0^I - Y_0^N \mid D=0\right] = 0 \tag{5.3}$$

The treatment of the treated group may not have an effect on the pre-treatment potential outcomes of both the treated and the control group. This assumption rules out that individuals anticipate the introduction of the treatment, which would spoil the treatment effect estimation. In the MOBIS experiment this assumption is satisfied as individuals were not given any information on a future treatment.

$$\text{Common Trends:} \quad \mathbb{E}\left[Y_1^N - Y_0^N \mid D=0\right] = \mathbb{E}\left[Y_1^N - Y_0^N \mid D=1\right] \tag{5.4}$$

As stated above this assumption is the basis of the DiD approach. It says that the treatment and control group would follow the same trend in absence of the treatment. Thus, any change in the post-treatment outcomes must be due to the treatment. This assumption also rules out that other policies or effects had different impacts on the two groups. One argument for this assumption to hold is the fact that treatment was randomly assigned and individuals were randomly drawn from the same population in the MOBIS experiment. Thus, on average, one would expect these individuals to follow the same trend, in absence of a treatment. Although, related to the argument of similar pre-treatment levels across treatment groups, this is different from what was analyzed by Table 4.3. One way to get an idea of the validity of the common trends assumption is by graphically comparing the pre-treatment outcomes of treated and control. If the common trend assumption is to hold, the pre-treatment outcomes should show similar trends across treatment groups as well. Figure 5.1 shows the average daily external costs per treated group, averaged over weekly intervals of the tracking period. Weekly averages were chosen, to lessen the noise embodied in individual days. Note that post-treatment trends cannot be compared, as the treatment would already cause a change in outcomes in these time periods.

Figure 5.1: Common trend plot



Note: Weekly average externalities per treatment group displayed. Externalities measured in CHF per day. The dashed lines indicate timing of treatment allocation.

When looking at Figure 5.1, it is important to keep in mind, that the plot ignores date-specific effects. However, as stated when discussing the randomization of the treatment in Section 4.2, one would expect these to affect the treated and control observations in similar ways, on average. Figure 5.1 shows that the control and treated group follow more or less the same trends before the treatment[9]. Nevertheless, there are some clear departures from the common trend visible. Looking at the aggregate of the three external costs in the upper left plot shows, that the pricing group tended to increase its externalities from week two to three, mostly driven by climate and congestion external effects. At the the same time, the information group deviated from a common trend with their health externalities. Looking at the overall picture and considering the date-fixed effects, which will be controlled for in the estimation, this assumption can also be assumed to hold[10].

[9]Interestingly, Figure 5.1 shows a clear negative slope of the trend for all three groups, which jumps back up a bit towards the end of the study. This shows, that also the control group reduced their external costs over the course of the experiment. One reason for this could be that individuals in the control group also experienced some sort of treatment by participating in the tracking study.

[10]In addition to the graphical approach chosen here, one could also run a (linear) ordinary least squares regression on the pre-treatment averages per treatment group. The coefficients, embodying the slopes of the linear time trends, should not differ significantly across the treatment groups.

These three assumptions are enough to estimate the ATET in the DiD setting, when the common trends assumption can be assumed to hold without conditioning on covariates Lechner (2011); Angrist and Pischke (2008).

$$\begin{aligned} ATET^{DiD} &= \mathbb{E}\left[Y_1^I \mid D=1\right] - \mathbb{E}\left[Y_1^N \mid D=1\right] \qquad (5.5)\\ &= \mathbb{E}\left[Y_1 \mid D=1\right] - \mathbb{E}\left[Y_0 \mid D=1\right] - \left(\mathbb{E}\left[Y_1 - Y_0 \mid D=0\right]\right) \qquad (5.6)\end{aligned}$$

As can be see, the $ATET^{DiD}$ could be estimated by simply calculating the average for each group and time period and then taking the differences. This works for both repeated cross section and panel data. The next part will cover how this can be done using regression formulations in the special case of panel data and with more than two time periods.

Estimation

There are many variants to estimate the ATET in the DiD setting described above. Given the data set generated by the MOBIS experiment, with a panel of 56 days and a likely presence of date and day-in-study fixed effects, the most straightforward approach is to use the fixed effects panel data estimator. With this estimator, one controls for the fixed effects, while comparing the average outcomes of the treatment groups on a day-specific basis Cameron and Trivedi (2005). Equivalently to Axhausen et al. (2021), the regression formulation used in this thesis to estimate θ^{Info} and θ^{Price}, i.e., the ATETs of the two treatment groups is given by

$$Y_{its} = c_0 + \theta^{Info} \cdot D_{its}^{Info} + \theta^{Price} \cdot D_{its}^{Price} + \sum_{k=1}^{K} \beta_k \cdot \left(X_{k,its} \cdot D_{its}^{\cdot}\right) + \mu_i + \mu_t + \mu_s + \epsilon_{its} \qquad (5.7)$$

where the subscripts $i \in \{1, \ldots, N\}$ and $t \in \{1, \ldots, 56\}$ denote individuals and day-in-study, respectively while subscript s stands for the date. μ_i, μ_t and μ_s are the respective fixed effects and the treatment dummies D_{its}^{Info} and D_{its}^{Price} take the value one if the individual has been exposed to the respective treatment at or before time t. Note that the fixed effects estimator applies the ordinary least squares regression on the demeaned variables Cameron and Trivedi (2005). It is important to note, that time constant, individual specific effects are differenced out as well in this approach. Thus, although the MOBIS data was randomized, small imbalances would be corrected using the estimation formulation in Equation 5.7. This has the consequence that it is not possible to include time constant variables in the regression. To analyze the treatment effect heterogeneity with respect to income, one thus includes the interaction of D_{its}^{Info} and D_{its}^{Price} with the two income dummies, *low income* and *high income*. This is represented by $\sum_{k=1}^{K} \beta_k \cdot (X_{k,its} \cdot D_{its}^{\cdot})$ in the equation above.

5.2 Quantile Treatment Effects

The DiD approach above is very useful to assess the average effect an intervention had on the treated population. However, there is a growing field in the literature which aims at expanding the analysis of treatments beyond their average effects. Quantile regressions consider the entire outcome distribution and allow to analyze what effect the treatment had on different quantiles of the distribution (henceforth denoted by τ). Using the same notation as before, let $F_{Y_t^I|D=1}$ denote the cumulative distribution function of the potential treated outcome of the treated population in period t. Similarly to the explanation in the DiD part, this distribution is observed in the data as $F_{Y_1|D=1}$. Likewise, the distribution functions $F_{Y_0|D=1}$, $F_{Y_1|D=0}$, and $F_{Y_0|D=0}$ are observed, and consequently identify the respective potential outcome distributions. From the identification of these potential distribution functions follows that all functions of these distributions are identified as well. Thus, also the inverse of the distribution function, called the quantile function, is identified (Callaway et al., 2018).

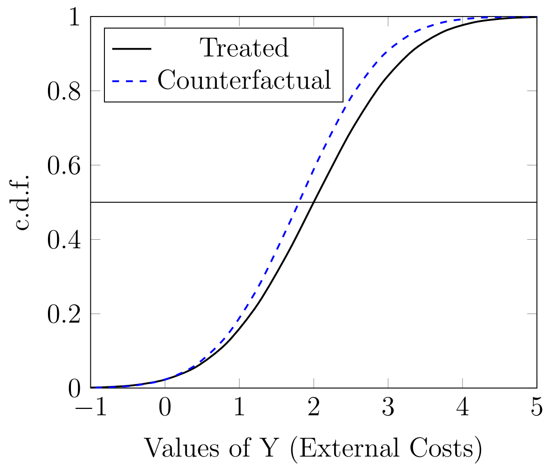
$$F^{-1}_{Y_t|D=d}(\tau) := \inf\left\{y \in \mathbb{R} : F_{Y_t|D=d}(y) \geq \tau\right\} \tag{5.8}$$

Quantile regression is based on these quantile functions of the outcomes, which it uses to identify the quantile treatment effects on the treated (QTET). Formally, the QTET is defined as the difference between the potential outcome levels of the treated group at the same quantile.

$$\text{QTET}(\tau) = F^{-1}_{Y_t^I|D=1}(\tau) - F^{-1}_{Y_t^N|D=1}(\tau) \tag{5.9}$$

Similarly to analyzing the ATET in the DiD setting, the fundamental problem of quantile regression is to identify the counterfactual distribution of the treated, had they not been treated, thus the second term of the above equation. Figure 5.2 illustrates the treatment effect measured by quantile regressions for a person which is at the median of the external cost distribution. The effect is shown by the horizontal distance between the potential treated distribution (black) and its counterfactual (blue). Shifting the line up or down shows the effect at other quantiles.

Figure 5.2: Example of two distribution functions (normally distributed)



Originally introduced by Koenker and Bassett (1978) in the cross-sectional setting, there is a growing literature applying the quantile regression idea to the setting found typically in DiD estimations (panel data, with observations prior and post potentially being treated). One quite known example is the Changes-in-Changes (CiC) estimator first proposed by Athey and Imbens (2006) and later extended to the case with covariates by Melly and Santangelo (2015). Another, estimator introduced by Athey and Imbens (2006) is the quantile difference-in-differences (QDID) approach. For an overview over the recent developments in the quantile regression literature see Koenker (2017), who also includes approaches in other settings, such as instrumental variables, non- or semi-parametric cross-section and dynamic panel data.

In this master thesis, the recently published approaches by Callaway et al. (2018) is used to estimate the missing counterfactual distribution. This approach is preferred over the CiC, as it exploits the panel structure of the MOBIS data instead of resorting to using it as repeated cross-section. Additionally, the application included in the paper of Callaway et al. (2018) very closely matches the goal of this thesis. In the following, their estimation approach is introduced.

5.2.1 Callaway et al. (2018)

Generally speaking, the approach by Callaway et al. (2018) uses a clever combination of a distributional common trends assumption and a new assumption based on copula functions. Although, being a sophisticated topic, the explanation of their estimator is presented in the following, again using the common separation between identification and estimation.

Identification
Similar to the explanation of the DiD approach and following Callaway et al. (2018) the potential outcome model with two treatment status $d \in \{0, 1\}$ and two time periods $t \in \{0, 1\}$ is used for this explanation. Outcomes are again defined to be generated as $Y_t = dY_t^I + (1-d)Y_t^N$, which at the same time rules out spillovers between the treatment groups. In addition to that, let $\Delta Y_1^N := Y_1^N - Y_0^N$ define the time difference between the untreated potential outcomes (for each treatment groups). Using this definition Callaway et al. (2018) introduce their first main identifying assumption.

$$\text{Distributional DiD:} \quad \Delta Y_1^N \perp D \tag{5.10}$$

In words, this assumption says, that treatment and control group experience the same change in the potential untreated outcomes. Thus, in absence of a treatment, both groups would experience the same trend. Importantly, the assumption does allow for treatment and control group having different initial levels in the potential outcome and only restricts them to experience a similar change over time. This assumption is very closely related to the common trend assumption made in the DiD setting in Equation 5.4, which can be reformulated as $\mathbb{E}[\Delta Y_t^N \mid D = 0] = \mathbb{E}[\Delta Y_t^N \mid D = 1]$.

With this assumption the distribution functions $F_{Y_0^N|D=1}$ (observed) and $F_{\Delta Y_0^N|D=1}$ (by the Distributional DiD assumption) are identified. However, this is not yet enough to identify the QTE, as the dependence between the original potential non-treated outcome in period 0, $(Y_t^N \mid D = 1)$, and the change in untreated potential outcome, $(\Delta Y_t^N \mid D = 1)$, is unknown. This is, why they impose a restriction on the joint distribution of these two random variables, defined as $F_{\Delta Y_1^N, Y_0^N|D=d}$. The restriction is imposed through the copula function[11] $C_{\Delta Y_1^N, Y_0^N|D=d}$ which can be understood as the function that couples the marginal distribution functions $F_{\Delta Y_1^N|D=d}$ and $F_{Y_0^N|D=d}$ to the multivariate, joint distribution function (Hofert et al., 2019). This is shown mathematically by Sklar's theorem (Sklar, 1959) which, applied to the present case, takes the form

$$F_{\Delta Y_1^N, Y_0^N|D=d}(\Delta y, y) = C_{\Delta Y_1^N, Y_0^N|D=d}\left(F_{\Delta Y_1^N|D=d}, F_{Y_0^N|D=d}\right) \tag{5.11}$$

for the values of $(\Delta y, y)$ supported by the data. Importantly, copula functions define the rank dependency between two (or more) variables, thus restricting the copula also restricts the dependence between the two variables (Genest and Favre, 2007). However, the restriction of the copula of two variables does only consider the rank of the variables and not the marginal distributions $F_{\Delta Y_1^N}$ and $F_{Y_0^N}$.

In Callaway et al. (2018) the restriction of the copula is given by the following assumption

$$\text{Copula Invariance:} \quad C_{\Delta Y_1^N, Y_0^N|D=1}(u, v) = C_{\Delta Y_1^N, Y_0^N|D=0} \tag{5.12}$$

which says, that the copula of (ΔY_1^N) and (Y_0^N) is the same for the treated and control group. Consequently, the Copula Invariance assumption allows to replace the unknown copula of the treated group with the observed copula of the control group and thus to identify the QTET. In order to check the validity of the two assumptions explained so far, Callaway et al. (2018) propose to look at the pre-treatment period. First, one could test them directly, similarly to what was done for the usual common trend assumption. Second, one could run the regression with a fake treatment assignment (also known as placebo testing).

Before turning to the identification of the QTET, it is important to understand the strength of using copulas to restrict the dependence between the original outcome and the change to the next period. No assumption about the actual form of the dependence is made. The copula-based assumption only requires the copulas to be the same, leaving it open if the two variables are perfectly dependent, independent or relate in any other form of dependence[12]. The size of the level change in the outcomes ΔY_0^N, is defined by the Distributional DiD assumption.

[11]For an illustrative introduction to the concept of copulas, the reader is referred to Genest and Favre (2007). Additionally, Hofert et al. (2019) gives a great introduction to the topic using R and their package *copula*.

[12]Technically, perfect dependence (either positive or negative) is given by the so called Frêchet-Hoeffding bounds (Genest and Favre, 2007). Independence is evoked by the independence copula.

Callaway et al. (2018) also need two technical assumptions on the data. Firstly, all variables involved in identification need to have a continuous distribution with “a compact support with densities uniformly bounded away from 0 and ∞ over the support” (Callaway et al., 2018). This ensures that the copulas are uniquely identified, as it rules out ties within the ranks of the variables (ΔY_1^N) and (Y_0^N) (Genest and Favre, 2007). In addition to that, Callaway et al. (2018) rely on the potential outcomes (Y_0^N, Y_0^I) and (Y_1^N, Y_1^I) each being cross-sectionally i.i.d. given treatment status d.

With the assumptions stated above, the following equation identifies the distribution of the treated, had they not been treated, which also determines the corresponding quantile function. Note that $\underline{1}$ is an indicator function of the event specified in brackets.

$$F_{Y_1^N|D=1}(y) = \mathbb{E}\left[\underline{1}\left\{\Delta Y_1 + F^{-1}_{Y_0|D=1}\left(F_{Y_0|D=0}\left(Y_0\right)\right) \leq y\right\} \mid D = 0\right] \tag{5.13}$$

The same authors also published another, very similar estimation approach in a even more recent study (Callaway and Li, 2019). Relying also on the Distributional DiD assumption, Callaway and Li (2019) take another approach to replace the unknown copula of the potential untreated outcomes of the treatment group. In this paper, they call their second main assumption the *copula stability* assumption, because they assume that the copula within a treatment group stays constant over time. Consequently, they use three time periods and infer the copula $C_{\Delta Y_1^N, Y_0^N|D=1}$ from the preceding two periods, thus from $C_{\Delta Y_0^N, Y_{-1}^N|D=1}$ which is fully observed. Both approaches yielded very similar results, which makes sense, because the treatment and control group should not systematically differ in the MOBIS experiment, as was discussed before. The approach by Callaway et al. (2018) was chosen, for the sole reason that it allows to simply split the data into two periods, pre- and post-treatment.

Estimation

Contrary to the DiD setting, the estimation of the method explained here is limited to this binary case. This means, that both the information and pricing treatment group are being compared to the control group individually. Additionally, the estimation approach of Callaway et al. (2018) cannot incorporate more than two time periods. To comply with these requirements the average daily outcome per individual over the entire pre and post treatment period will be used to construct the two observations. This means, that date specific effects are ignored. Apart from this, the estimation procedure is relatively straightforward.

From Equation 5.13 is known that estimates of $F^{-1}_{Y_0|D=1}$ and $F_{Y_0|D=0}$ are required. These can be estimated using the empirical distribution function which here is defined as

$$\hat{F}_{Y_t|D=d} = \frac{1}{n^{(d)}} \sum_{i=1}^{N} \underline{1}\left\{Y_{it} \leq y\right\} \cdot \delta_i^{(d)} \tag{5.14}$$

where $\delta_i^{(d)} := \underline{1}\left\{D_i = d\right\}$ is an indicator function taking the value one when treatment is d. Furthermore, $n^{(d)}$ is the number of individuals receiving treatment d and N is the total number of individuals in the sample. The empirical distribution function is a step-function that computes the cumulative density left of a given threshold y. It is a non-parametric estimator of the true underlying distribution function (Hofert et al., 2019). The distribution functions $F_{Y_0|D=1}$ and $F_{Y_0|D=0}$ are estimated using the observed data as $\hat{F}_{Y_0|D=1}$ and $\hat{F}_{Y_0|D=1}$, respectively. After inverting $\hat{F}_{Y_0|D=1}$, to receive the needed quantile counterpart, one can finally estimate the untreated potential distribution function of the treated. As can be seen below, this is again done using the empirical distribution function.

$$\hat{F}_{Y_1^N|D=1}(y) = \frac{1}{n^{(0)}} \sum_{i=1}^{n} 1 \left\{ \Delta Y_{i1} + \hat{F}^{-1}_{Y_0|D=1} \left(\hat{F}_{Y_0|D=0} \left(Y_{i,0} \right) \right) \leq y \right\} \delta_i^{(0)} \tag{5.15}$$

To receive estimates for the QTET one now has to invert $\hat{F}_{Y_1^N|D=1}$ and estimate the observable potential quantile distribution of the treatment group in the treated potential state.

$$QTET(\tau) = \hat{F}^{-1}_{Y_1^I|D=1}(\tau) - \hat{F}^{-1}_{Y_1^N|D=1}(\tau) \tag{5.16}$$

Hence, that Callaway et al. (2018) introduced their estimator specifically with the possibility to include covariates to correct for a lack of common trend. As the common trend assumption was assumed to be holding in this thesis, this option was not used. For that reason the explanation has been simplified in this thesis, to ease the notational burden. Callaway et al. (2018) additionally condition every statement above on $X = x$. Therefore this estimation approach allows to condition the estimate of the QTET on a subgroup of the population with common history or characteristics x. In Section 6.3 this approach will be chosen as well, and the QTETs are computed for the three income groups separately. Fortunately, Callaway et al. (2018) maintain a R package, called *qte*, which implements their estimators and also includes a substantial set of explanatory resources.

6 Analysis

6.1 Descriptive Statistics

This section presents some descriptive statistics on the differences between the income group in combination with outcome variables. Table 6.1 shows the averages of the main socio-demographic variables for each of the three income groups.

Table 6.1: Average demographics per income group

	Low Income	p-val	Reference	p-val	High Income	p-val
Individuals	742		1260		1233	
Male	0.43	0.03	0.49	0.00	0.57	0.00
German	0.73	0.01	0.65	0.23	0.65	0.00
French	0.24	0.17	0.29	0.10	0.24	0.47
Age < 30	0.39	0.00	0.17	0.00	0.17	0.00
$30 \leq$ Age ≤ 55	0.49	0.00	0.75	0.61	0.58	0.00
Age < 55	0.12	0.00	0.08	0.01	0.25	0.00
Primary Education	0.11	0.01	0.08	0.00	0.04	0.00
Secondary Education	0.63	0.00	0.43	0.00	0.37	0.00
Tertiary Education	0.26	0.00	0.48	0.00	0.59	0.00
Household Size	3.33	0.00	4.23	0.00	2.67	0.18
Full-time Employed	0.69	0.23	0.66	0.00	0.77	0.00
Equivalized Income	2,513	0.00	4,878	0.00	8,845	0.00

Notes: Average values per income group. Columns denoted with “p-val” indicate the p-value of the difference between its neighboring columns. A value below 0.05 indicates a significant difference at the 5%-significance level. Equivalized income denoted in CHF.

From the table, one can see that individuals in the low-income groups were on average younger and more likely to be female and German-speaking. They were also more likely to have only attended primary education or secondary education but much more unlikely to have attended university. They were not significantly less likely to have a full-time job as the reference income group. The high-income group lives in smaller households and is more likely to be full-time employed.

Thanks to the setup of the MOBIS experiment, the different income groups can be compared in their mobility behavior before being treated as well. Table 6.2 begins with showing the share of each income group that owns a bike or a car. It can be seen that higher incomes are much more likely to own a car, while the share of car owners generally is high. This is likely due to the entry requirements of the MOBIS study. Individuals with low incomes are more likely to use the public transport system (train or local public transport) more than three times per week and less likely to travel by car more than three times per week compared to the other income groups.

Table 6.2: Average externalities per income group

	Low Income	p-val	Reference	p-val	High Income	p-val
Individuals	742		1260		1233	
Owns Bike	0.66	0.00	0.81	0.72	0.74	0.00
Owns Car	0.80	0.00	0.89	0.01	0.93	0.00
Regular Pub. Trans.	0.34	0.00	0.30	0.56	0.26	0.04
Regular Car	0.69	0.05	0.71	0.18	0.75	0.03
Dist. Total	43,385	0.27	44,682	0.00	49,903	0.00
Dist. Car	31,900	0.21	31,823	0.00	36,734	0.00
Dist. Bike	563	0.10	893	0.89	733	0.12
Dist. Walking	1,944	0.02	1,920	0.53	1,845	0.96
Dist. Pub. Trans.	8,977	0.89	10,046	0.01	10,591	0.00
Ext. Cost Car	3.95	0.13	3.96	0.00	4.64	0.00
Ext. Cost Bike	0.04	0.11	0.06	0.88	0.05	0.12
Ext. Cost Walking	-0.22	0.02	-0.21	0.51	-0.21	0.97
Ext. Cost Pub. Trans.	0.27	0.78	0.33	0.09	0.30	0.01
Congestion Ext. Cost	0.90	0.03	0.98	0.00	1.18	0.00
Climate Ext. Cost	0.81	0.65	0.80	0.00	0.91	0.00
Health Ext. Cost	2.33	0.12	2.35	0.00	2.70	0.00
Total Ext. Cost	4.04	0.09	4.13	0.00	4.79	0.00

Notes: Average transport related pre-treatment values per income group. Columns denoted with "p-val" indicate the p-value of the difference between its neighboring columns. A value below 0.05 indicates a significant difference at the 5%-significance level. All observations below "Regular Car" are averages per day in the pre-treatment period. Distances are denoted in meters, external costs in CHF.

Table 6.2 also shows the average distances and external costs per mode class and per day over all individuals in this income group in the pre-treatment phase. One can see that the wealthiest income group did travel the furthest on average per day. This was driven mainly by larger average distances with a car or using public transport. Interestingly, all three groups travel about the same distance by bike on average, with this mode being by far the one with the lowest average distance. The differences in average distance traveled per day across the income groups are mostly matched by the differences in average external costs caused by this behavior.

The last block in Table 6.2 shows the average external costs per income group, subdivided into the four external cost categories considered in this thesis. There is an apparent trend that the higher the income, the higher the external costs imposed upon society for all three external costs. However, this trend is much more pronounced at the upper-income level, and the difference between the low and middle-income groups is only significant for the congestion externality.

6.2 Average Treatment Effects

After the descriptive statistics of the income groups, this section presents the results of the Difference-in-Differences (DiD) estimator introduced in Section 5.

Table 6.3 shows the regression results where models 1, 4, and 7 show the average treatment effects on the treated (ATET). Importantly, the reported effects are absolute changes and denoted CHF of marginal external costs per day. Models 2, 5, and 8 analyze the ATET conditional on the two income groups and their interactions with the treatment, and 3, 6, and 9 report the CATET conditional on income, education, gender, and age. These additional covariates were chosen, as Table 6.1 has shown significant differences between the income groups in these variables. Including these variables could thus allow a cleaner estimate of the income group coefficients.

Interpreting the results of models 1, 4, and 7 shows that the point estimate for the information treatment is very small and not significantly different from zero at the 10%-significance level for all three categories. The pricing treatment leads, on average and ceteris paribus, to a significant reduction in all three categories of external costs, with the effect being the strongest in health-related externalities.

From the estimations of models 2, 5, and 8, one can learn about the effect heterogeneity with respect to income in the average treatment effects on the treated. To interpret this correctly, note that the two topmost rows (*Info* and *Pricing*) now denote the effect of the reference income group. The coefficients for the interactions are departures from this effect. Consequently, the total effect of the information treatment for the low-income group is the sum of *Info* and *Low Inc. x Info*.
The reference group with an equivalence income between 4,000 and 6,500 CHF per month shows about the same point estimates as were found for the overall population (see column 1, 4, and 7). However, most of the effects are not significantly different from zero at the 10%-level. Especially for the pricing treatment, this is noteworthy, as a highly significant effect was measured for the overall population. The low-income group with less than 4,000 CHF of equivalized income showed no significant departures from these effects. However, they tend to react less to the information treatment and more to the pricing treatment. Notably, this is not true for the congestion externalities to which low-income individuals responded with lower reduction than the reference category. Lastly, those individuals earning more than 6,500 CHF per month showed virtually no departure from the effect on the reference group for the information treatment. However, the pricing treatment effect had a significant effect on their congestion and external climate costs. The coefficient of health externalities was even larger but also less precise and not significantly different from zero, on average. With the mostly significant departures from the reference groups and the quite substantial coefficients, one can say that the members of this income group drive the overall significant effect of the pricing treatment.

This last statement is supported by the results in columns 3, 6, and 9. They show the effect heterogeneity with respect to a multitude of covariates at the same time. The reference

Table 6.3: Fixed effects regression output for congestion, climate and health

	Congestion			Climate			Health		
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Info	-0.02	-0.03	-0.008	-0.02	-0.02	0.005	-0.05	-0.09	-0.05
	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)	(0.05)	(0.06)	(0.08)
Pricing	-0.07***	-0.05*	-0.02	-0.04**	-0.007	0.02	-0.12**	-0.05	0.02
	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)	(0.05)	(0.07)	(0.09)
Low Inc. x Info		0.05	0.06		0.02	0.02		0.05	0.04
		(0.04)	(0.04)		(0.03)	(0.03)		(0.08)	(0.08)
Low Inc. x Pricing		0.07*	0.09**		-0.04	-0.03		-0.11	-0.09
		(0.04)	(0.04)		(0.03)	(0.03)		(0.09)	(0.09)
High Inc. x Info		-0.0003	0.008		-0.01	-0.009		0.07	0.07
		(0.04)	(0.04)		(0.03)	(0.03)		(0.07)	(0.08)
High Inc. x Pricing		-0.09**	-0.09**		-0.06*	-0.05*		-0.13	-0.12
		(0.04)	(0.04)		(0.03)	(0.03)		(0.08)	(0.08)
Low Educ. x Info			0.03			-0.08*			-0.25*
			(0.06)			(0.05)			(0.14)
Low Educ. x Pricing			0.11*			0.04			0.11
			(0.06)			(0.06)			(0.17)
High Educ. x Info			0.04			-0.02			-0.05
			(0.03)			(0.03)			(0.07)
High Educ. x Pricing			0.06*			-0.002			-0.03
			(0.04)			(0.03)			(0.07)
Male x Info			-0.10***			-0.02			-0.004
			(0.03)			(0.02)			(0.06)
Male x Pricing			-0.10***			-0.02			-0.06
			(0.03)			(0.03)			(0.07)
Young x Info			0.007			-0.004			0.04
			(0.04)			(0.03)			(0.08)
Young x Pricing			-0.07			-0.06**			-0.15*
			(0.04)			(0.03)			(0.08)
Old x Info			0.03			-0.01			-0.04
			(0.04)			(0.03)			(0.09)
Old x Pricing			0.004			-0.04			-0.02
			(0.05)			(0.04)			(0.09)
R^2	0.27833	0.27848	0.27873	0.22990	0.22994	0.23001	0.23375	0.23378	0.23385

Notes: DiD estimation results for the three types of external cost categories. The coefficients are denoted in Swiss francs (absolute change). All regressions include *individual*, *date* and *day-in-study* specific effects and used 152,429 observations. *Clustered (user_id) standard-errors in parentheses.*
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

group, shown in the first two rows, consists of women with an equivalized income between 4,000 and 6,500 CHF per month, which have only completed secondary school and are between 30 and 55 years old. As can be seen from the first two rows, this subgroup of the reference-income group shows almost no response to the treatments. The point estimates of the other two income groups show to be very similar in size compared to before (see 2, 5, and 8). This shows that the differences in treatment response between the income groups do not depend on education, age, and gender. However, the absolute effects explained by the

three income groups tend to do, as the base group effect did change.
The remaining coefficients can be interpreted similarly as was done for the income groups and will not be discussed thoroughly. Nevertheless, the fact that male individuals reduced their external congestion costs by 0.1 CHF more than women seems worth highlighting. This is true for both treatments. Apart from this, attending higher or lower education than secondary schooling or being above 55 years old (*old*) does not seem to introduce significant heterogeneity in treatment response. Only those below the age of 30 (*young*) showed significant effect heterogeneity to the pricing treatment with their climate and external health costs.

After analyzing the effects on the three external cost categories, Table 6.4 summarizes the (C)ATETs on the sum of the marginal external costs to assess the total effect on external costs. Model (1) again shows the ATET of the two treatments. Unsurprisingly, the coefficient for the information treatment shows to be insignificantly different from zero for the total external costs as well. Charging the individuals their marginal external costs, however, has a significant overall effect. On average, the participants reduced their external costs by 0.23 CHF per day, which, given the average total external costs in the pre-treatment phase of about 4.51 CHF (see Table 4.3), is a reduction by 5.1 percent.

For the second model, the interaction of high income and pricing shows a significant and strong effect, which also remains stable when including more covariates (model 3). This confirms the overall trend, seen before, that (treated) individuals of this income group drive the effect of the pricing treatment. However, when comparing the treatment effects for the reference group in models (2) and (3), it is apparent that the other variables in (3) explain some part of the heterogeneity as well. Especially the young seem to be reacting significantly different than those aged between 30 and 55, as always on average and all other effects held constant.

Table 6.5 gives an overview over the total and relative effects for the three income groups. The high income group also shows to have the largest relative change, despite having more external costs to begin with. However, also the low income group is shown to have reacted considerably in relative terms. This statements have to be taken with caution, as no standard errors are provided.

Besides the presented estimation results, numerous other models have been estimated to test the robustness of the effect. Leaving out the date-specific effects or including region-fixed effects did not show to change the estimates a lot. Orthogonalising the data, also known as purging, did not change the results at any meaningful scale. In general, if the daily external costs are averaged over a longer period (e.g., week), the ATETs point in the same direction but are less pronounced and consequently less significant. The author also ran a regression to compare post-treatment outcomes only, which corresponds to the selection on observables approach, shortly described in the methodology part. When controlling for pre-treatment outcomes, the effects of this approach were similar but slightly smaller as under DiD assumptions. In addition to these extensions, Axhausen et al. (2021) included the weather in the regression, to get a more precise estimate. However, they did only observe a very small change in the estimates and standard errors when controlling for weather.

Table 6.4: DiD estimation output for total external costs

	Total External Costs		
Model:	(1)	(2)	(3)
Info	-0.09	-0.14	-0.06
	(0.07)	(0.09)	(0.13)
Pricing	-0.23***	-0.10	0.02
	(0.08)	(0.11)	(0.14)
Low Inc. x Info		0.13	0.12
		(0.13)	(0.14)
Low Inc. x Pricing		-0.08	-0.03
		(0.15)	(0.15)
High Inc. x Info		0.05	0.07
		(0.12)	(0.12)
High Inc. x Pricing		-0.27**	-0.26**
		(0.13)	(0.13)
Low Educ. x Info			-0.30
			(0.21)
Low Educ. x Pricing			0.26
			(0.26)
High Educ. x Info			-0.03
			(0.11)
High Educ. x Pricing			0.04
			(0.12)
Male x Info			-0.12
			(0.10)
Male x Pricing			-0.18
			(0.11)
Young x Pricing			-0.28**
			(0.14)
Young x Info			0.04
			(0.12)
Old x Pricing			-0.05
			(0.15)
Old x Info			-0.02
			(0.15)
R^2	0.24096	0.24101	0.24110

Notes: DiD estimation results for total marginal external costs. The coefficients are denoted in Swiss francs (absolute changes). All regressions include *individual*, *date* and *day-in-study* specific effects and used 152,429 observations. *One-way (user_id) standard-errors in parentheses. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Table 6.5: Total and relative effects per income

Total External Costs			
Coeff.	Tot. Effect	Pre	Rel. Effect
-0.14	-0.14	4.13	-3.4
(0.09)			
-0.1	-0.1	4.13	-2.4
(0.11)			
0.13	-0.01	4.04	-0.2
(0.13)			
-0.08	-0.18	4.04	-4.5
(0.15)			
0.05	-0.09	4.79	-1.9
(0.12)			
-0.27	-0.37	4.79	-7.7
(0.13)			

Notes: Total and relative effects of the treatments on the three income groups in model (2). Pre-treatment values as presented in Table 6.2. Tot. Effect is the sum of the interactions and reference group. Relative effects in percent, all other effects in CHF.

6.3 Quantile Treatment Effects

This section presents the quantile treatment effects on the treated (QTET) of the estimator by Callaway et al. (2018), using bootstraps with 1,000 iterations to receive standard errors. The results are mainly presented in a graphical manner. First, the unconditional QTET for the information and pricing treatment is described. Then, the results are shown conditionally on the three income groups. Note, that no other covariates are considered in this part and possible date-fixed effects have been ignored.

First, the general interpretation of the figures used to present the QTETs in this section is explained. The solid line represents the QTET at any given quantile of the outcome, with the dots being the actual estimated effects. Dashed lines represent the 95% confidence bands calculated for each quantile. If the area between the dashed lines does not cover the zero-effect line, there is a significant treatment effect at this quantile of the outcome and for this population. To show an example, consider the graph displaying the QTET of the pricing treatment on congestion at 19 quantiles (0.05 to 0.95) in Figure 6.1. It can be seen that individuals at the 0.15, between 0.45 and 0.7, and at the 0.8 quantiles of the outcome distribution significantly reduced their congestion externalities. There is also a trend visible that the more congestion externalities were produced, the stronger the effect tended to be. However, those who had the most congestion externalities responded less and showed great variance, spoiling any significance of the measured average effect at these higher quantiles.

Applying this style of interpreting to the figures on the next page shows that not many individuals have responded significantly to the information treatment. In fact, the treatment effects seem to be rather homogeneous over the quantiles of the three external cost distributions, which can be inferred from its horizontal appearance. Towards the upper quantiles (the upper 20% of individuals in terms of external costs), the effect shows to vary a lot. Especially the effect on external health costs shows a sharp increase, although not being significant whatsoever. Looking at the aggregated external cost outcome, one can detect a small significant effect just below the median.

For the pricing treatment, the results are quite similar in terms of the overall trends. However, the effects are stronger at most quantiles (hence the scaling of the y-axis) and are also more significant. Primarily, individuals with about median congestion and external health costs reacted significantly with these externalities to the treatment. Individuals with low external costs did not react significantly, and for individuals at the highest quantiles, the effects become again intractable. This gives the QTET a slight u-shape. On the summed level, this trend is confirmed, as seen at the bottom of the page.

Figure 6.1: QTET for entire population and both treatments

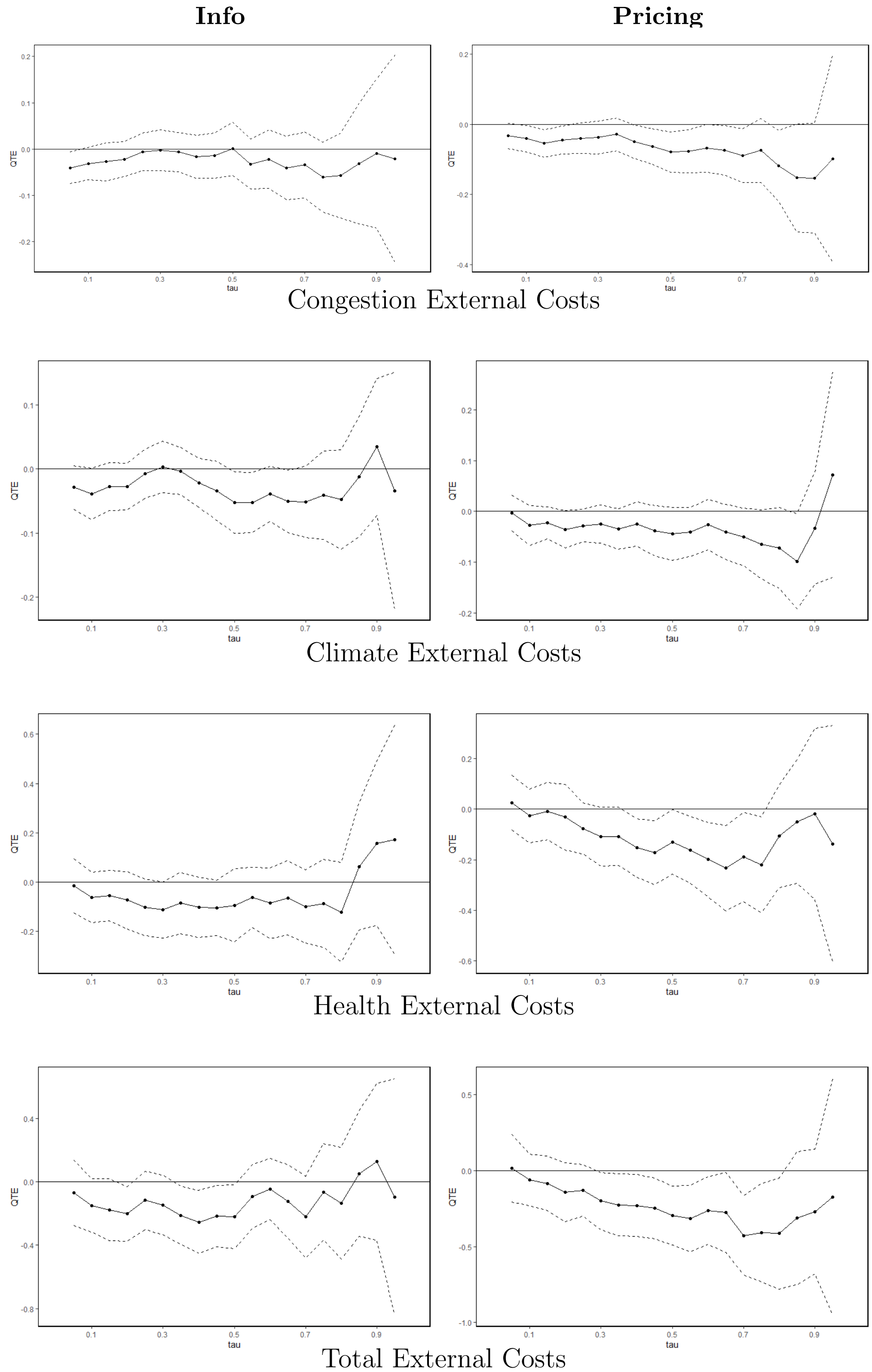


Table 6.6 show the same QTETs as discussed above in the form of a table, to show the actual size of the effects per quantile. The interpretation of the effects, however, is the same as for the graphical results which is why it is not repeated for this table.

Table 6.6: QTET estimation output for entire sample

	Information				Pricing			
τ	Total	Congestion	Climate	Health	Total	Congestion	Climate	Health
0.05	-0.07	-0.04**	-0.03*	-0.01	0.02	-0.03*	-0.00	0.03
	(0.10)	(0.02)	(0.02)	(0.06)	(0.11)	(0.02)	(0.02)	(0.05)
0.10	-0.15*	-0.03*	-0.04*	-0.06	-0.06	-0.04**	-0.03	-0.03
	(0.09)	(0.02)	(0.02)	(0.05)	(0.09)	(0.02)	(0.02)	(0.05)
0.15	-0.18*	-0.03	-0.03	-0.05	-0.08	-0.05***	-0.02	-0.01
	(0.10)	(0.02)	(0.02)	(0.05)	(0.09)	(0.02)	(0.02)	(0.06)
0.20	-0.20**	-0.02	-0.03	-0.07	-0.14	-0.05**	-0.04*	-0.03
	(0.09)	(0.02)	(0.02)	(0.06)	(0.10)	(0.02)	(0.02)	(0.07)
0.25	-0.12	-0.01	-0.01	-0.10	-0.13	-0.04*	-0.03*	-0.08
	(0.09)	(0.02)	(0.02)	(0.06)	(0.09)	(0.02)	(0.02)	(0.05)
0.30	-0.15	-0.00	0.00	-0.11*	-0.20**	-0.04	-0.02	-0.11*
	(0.10)	(0.02)	(0.02)	(0.06)	(0.10)	(0.02)	(0.02)	(0.06)
0.35	-0.21**	-0.01	-0.00	-0.08*	-0.23**	-0.03	-0.03*	-0.11*
	(0.09)	(0.02)	(0.02)	(0.06)	(0.10)	(0.02)	(0.02)	(0.06)
0.40	-0.25**	-0.02	-0.02	-0.10	-0.23**	-0.05**	-0.02	-0.15***
	(0.10)	(0.02)	(0.02)	(0.06)	(0.10)	(0.02)	(0.02)	(0.06)
0.45	-0.22**	-0.01	-0.03	-0.10*	-0.25**	-0.06**	-0.04	-0.17***
	(0.10)	(0.02)	(0.02)	(0.06)	(0.10)	(0.03)	(0.03)	(0.06)
0.50	-0.22**	0.00	-0.05**	-0.09	-0.30***	-0.08***	-0.04*	-0.13**
	(0.10)	(0.03)	(0.02)	(0.08)	(0.10)	(0.03)	(0.03)	(0.07)
0.55	-0.09	-0.03	-0.05**	-0.06	-0.32***	-0.08**	-0.04	-0.16**
	(0.10)	(0.03)	(0.02)	(0.06)	(0.11)	(0.03)	(0.02)	(0.07)
0.60	-0.05	-0.02	-0.04**	-0.08	-0.26**	-0.07*	-0.03	-0.20***
	(0.10)	(0.03)	(0.02)	(0.07)	(0.11)	(0.03)	(0.03)	(0.07)
0.65	-0.12	-0.04	-0.05**	-0.06	-0.28**	-0.07**	-0.04	-0.23***
	(0.12)	(0.03)	(0.02)	(0.08)	(0.13)	(0.04)	(0.03)	(0.09)
0.70	-0.22*	-0.03	-0.05**	-0.10	-0.43***	-0.09**	-0.05*	-0.19**
	(0.13)	(0.04)	(0.03)	(0.08)	(0.13)	(0.04)	(0.03)	(0.09)
0.75	-0.06	-0.06	-0.04	-0.09	-0.41**	-0.07	-0.06*	-0.22**
	(0.16)	(0.04)	(0.04)	(0.09)	(0.16)	(0.05)	(0.03)	(0.10)
0.80	-0.14	-0.06	-0.05	-0.12	-0.41**	-0.12**	-0.07*	-0.11
	(0.18)	(0.05)	(0.04)	(0.10)	(0.19)	(0.05)	(0.04)	(0.10)
0.85	0.05	-0.03	-0.01	0.06	-0.31	-0.15*	-0.10**	-0.05
	(0.20)	(0.07)	(0.05)	(0.13)	(0.22)	(0.08)	(0.05)	(0.12)
0.90	0.13	-0.01	0.03	0.16	-0.27	-0.15*	-0.03	-0.02
	(0.25)	(0.08)	(0.05)	(0.17)	(0.21)	(0.08)	(0.06)	(0.17)
0.95	-0.09	-0.02	-0.03	0.17	-0.17	-0.10	0.07	-0.14
	(0.38)	(0.11)	(0.09)	(0.24)	(0.40)	(0.15)	(0.10)	(0.24)
ATET	-0.11	-0.02	-0.02	-0.06	-0.20**	-0.07**	-0.03	-0.10**
	(0.08)	(0.02)	(0.02)	(0.05)	(0.08)	(0.03)	(0.02)	(0.05)

*Notes: Quantile regression output for entire sample. Standard errors (in parentheses) calculated with 1,000 bootstrap iterations. Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

Conditional QTET

As the last part of the analysis, the results of the quantile regressions are presented for the three income groups. As there are four outcome variables, two treatments, and three income groups, the presentation of these results is quite tedious. For this reason, the next two pages each present the results for one treatment variable, starting with the information treatment. Each page is then subdivided into twelve parts, where columns group the income categories and rows represent the external cost categories (in the same order as for the unconditional QTET). Standard errors are again received from bootstrapping the estimator of Callaway et al. (2018) with 1,000 iterations.

The information treatment was shown to have little effects on average (see Section 6.2) and for the overall population's quantiles. On page 34, these results can seen to be confirmed for all three income groups. Mostly insignificant and quite homogeneous QTETs are measured for all three groups, again with higher variance in the upper quantiles. Interestingly, low-income individuals with high external costs seem to react by increase their externalities. For those causing about median-level total external costs the information treatment is almost significant, mostly driven by significant reductions in external climate costs. For individuals earning more than 4,000 CHF per month and person virtually no reaction to the information treatment was measured.

For the pricing treatment, the overall trend found in the entire population was present in the three income groups as well. In general, the treatment had the strongest effect on individuals with about median-level external costs, and the effect varied a lot in the upper quantiles. Compared to the reference group, individuals with low or high incomes tended to react more to the treatment. For the low-income group, a significant reduction (as always at the 5%-level) was observed in external climate and health costs, at or slightly below the median outcome level. The high-income group showed a similar trend for all three external cost categories, but for congestion, all quantiles below the median outcome showed significant responses. For total marginal external costs, low-income individuals showed a u-shaped response with no effect at the lowest quantiles and even a positive (but insignificant) average effect at the highest quantiles. The reference group did not respond to the pricing of their externalities over almost all quantiles. For high-income individuals, the average treatment effect seems to be largely driven by the effect on persons with median-level total external costs.

The tables, which concretize the QTETs displayed hereafter can be found in appendix II.

Figure 6.2: Conditional QTET - Information Treatment

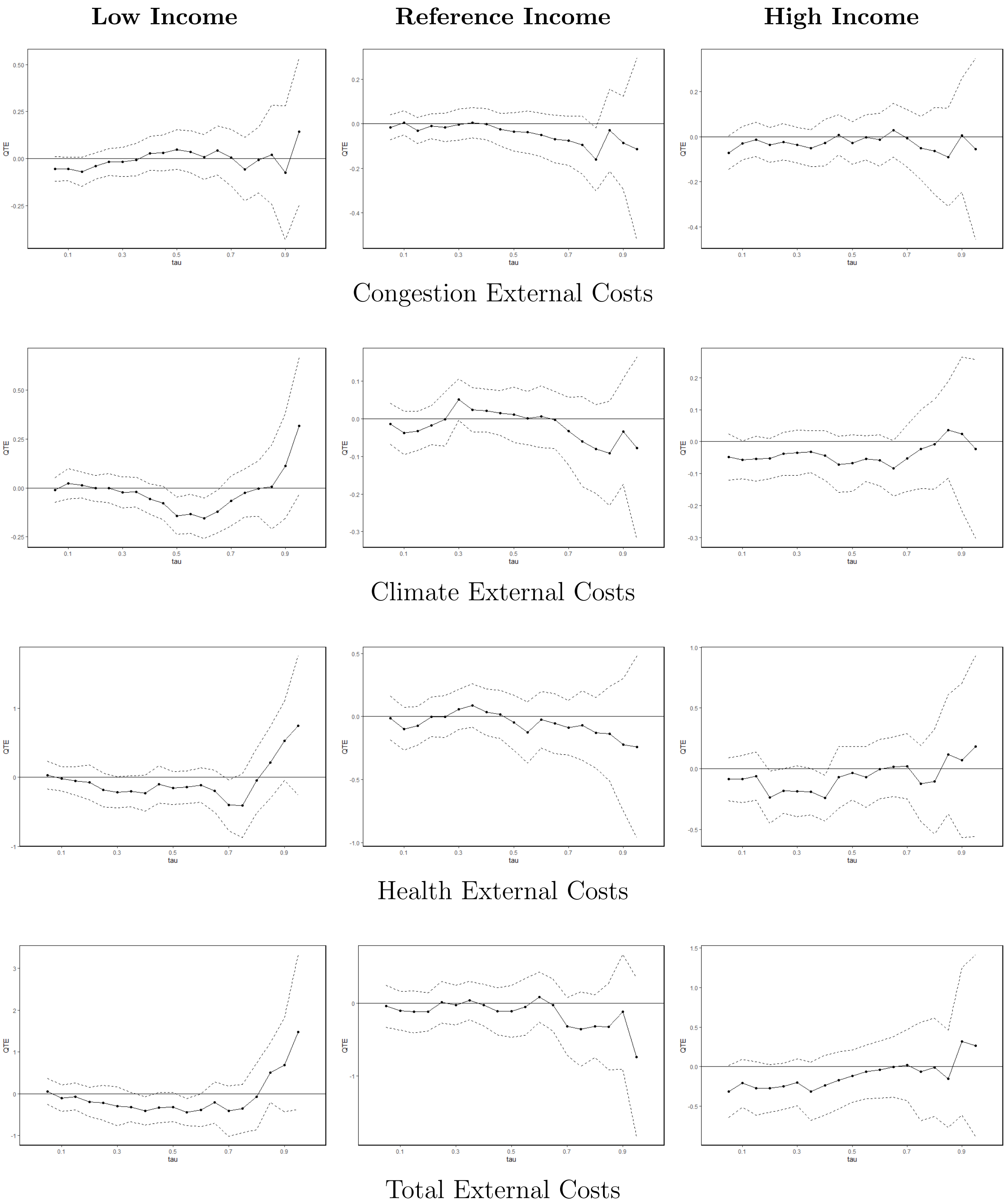
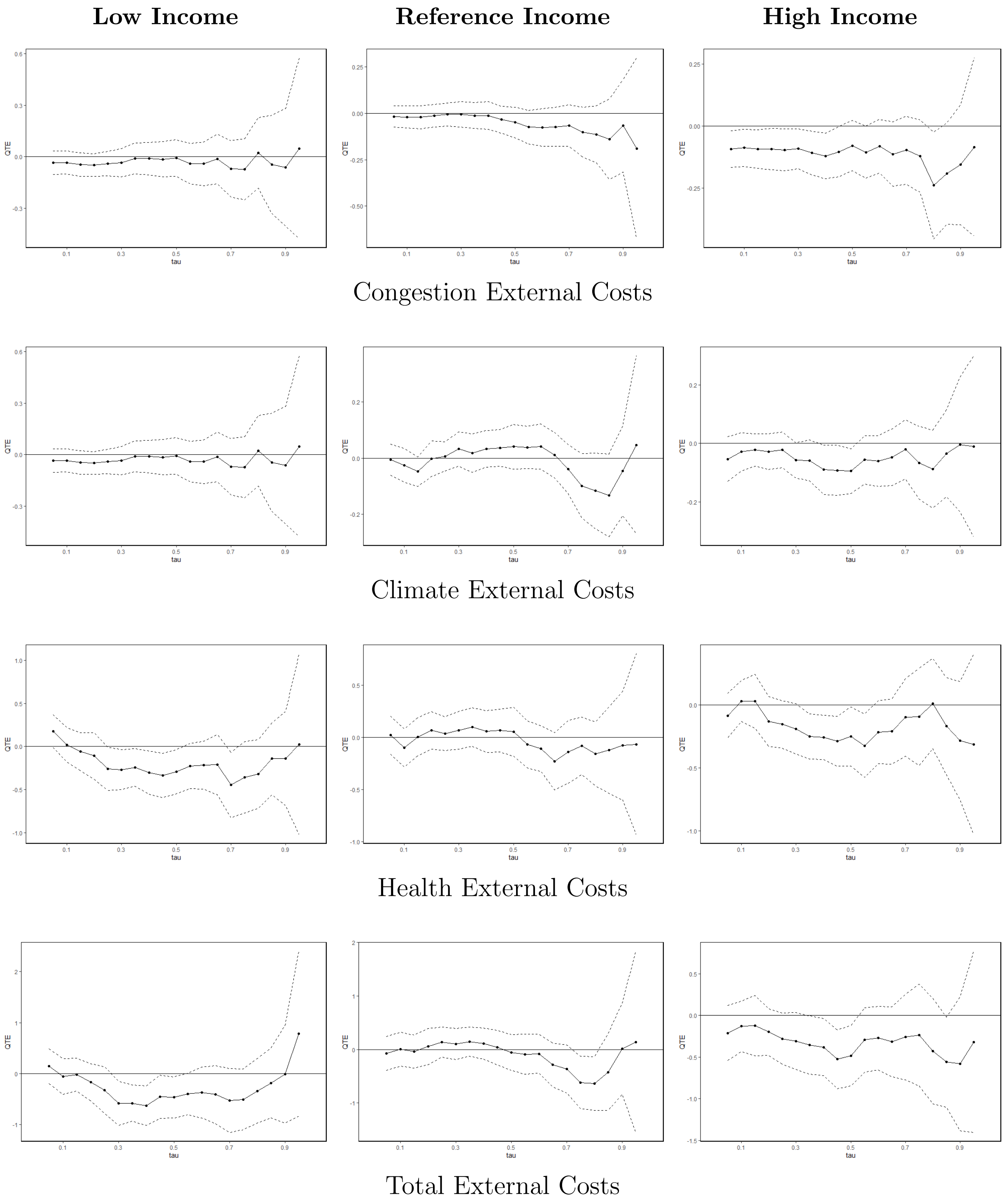


Figure 6.3: Conditional QTET - Pricing Treatment



7 Discussion

Overall, the descriptive and average treatment analysis confirmed the findings made in Axhausen et al. (2021). The application of quantile regression did allow some additional insights into which study participants drive the average treatment effects. The following will go through the different parts of the analysis, discuss the findings and highlight the key implications of this thesis.

At the beginning of the preceding chapter, it was shown that individuals with low incomes tend to be less educated and more likely to be female. In addition to that, there seems to be a positive correlation between income level and average external costs of mobility. This is in line with the overall Swiss population, according to Swiss Federal Council (2016). The MOBIS experiment was requiring individuals to travel by car at least twice per week. This could be a driving factor for the surprisingly low average distances by bicycles reported in Table 6.2.

As was already found in Axhausen et al. (2021), charging the marginal external costs of mobility from the study participants was shown to significantly affect all three external cost categories. The reduction in health-related externalities was the strongest, followed by congestion and could be shown to be driven mostly by individuals with higher incomes. The latter reduced their total marginal external costs by 0.27 CHF more than the reference of -0.1 CHF. This results in a 7.7% reduction in total externalities for this group. The low income group showed to react with a relative reduction by 4.5%. The lower response by low incomes to a tax is somewhat surprising, as one would expect this group to care most about financial incentives. Unfortunately, this thesis can make no statements about the reason for this relatively low response. The strong reaction with external health costs could hint at individuals having responded with their car travel, as this is the mode with the highest costs per kilometer in this category (see Table 4.1. Further analysis could check this hypothesis in the data.

The estimation results in this thesis were based on the newly constructed equivalence income as a measure of monthly income. Nevertheless, the results are very similar to those of Axhausen et al. (2021), who used household income as their income variable. They had found that individuals with high household income respond to the pricing treatment with a 7% decrease in total external costs. This is only slightly smaller than when using the equivalence income definition in this thesis. Given the considerable change in income group assignment described in Table 4.5, this similarity could be seen as a surprising result.

It seems worth noting that although causal effects were estimated in this thesis, no statements about the reason why different sub-groups react differently to the treatments can be made. For example, the fact that men respond significantly more with their congestion externalities does not demonstrate that women do care less about money or their congestion externalities. There could be a fundamental difference in what kind of trips women and men tend to do or some factors which prevent women from changing their mobility behavior. This reasoning can, of course, be applied to the income groups as well. Maybe, lower incomes are not able to respond to the treatments. One reason for this could be that lower incomes might have jobs with stricter working hours, which prohibits them from shifting their commuting time away from the congestion peaks.

The estimation of the quantile treatment effects on the treated allowed to look beyond the average effects of the treatment. For the information treatment, however, the treatment effect turned out to be very homogeneous across most quantiles. This was also true for all three income groups. However, for the pricing treatment a u-shape was apparent for most externalities of both the low and high-income groups. This was mainly driven by the reductions made in external health costs. The interpretation of this u-shape is, that individuals with low external costs tend not to respond as much when required to pay for their marginal external costs. This seems plausible, as those with low external costs, do not have as much room for a further decrease.Additionally, their newly charged external costs are low in absolute terms. As expected, the treatment effect got stronger with higher (total) external costs, relative to the sample. The highest treatment effect was measured for those individuals at the 0.7 quantile of total external costs. Interestingly, this almost linear downward trend turns and, starting from the 0.8 quantiles, vanishes within the variance of the effect.

One explanation for the treatment effect getting weaker for those with the most substantial external costs could be that these individuals are not able to react to the treatment. At the base of this argument lies that these individuals possibly have a reason for their high external costs, which they cannot change as a response to the treatment. One example of this could be that of commuters at rush hour. These individuals commute during the most dreadful time of the day, to begin with. Therefore, one could expect that they are doing this not completely voluntarily but because they are expected to arrive at work at or before a fixed time. A complementary explanation for the high variance towards the higher quantiles could be that the lower bound consist of individuals who have a strong incentive to reduce their high absolute level of external costs. Consequently, these are the individuals continuing the trend of higher treatment response with higher external costs. This begs the question who the individuals could be that actually increase their already high external costs as a response to the treatments.

One reason for this could be that paying for the external damage of one's mobility behavior could lessen the moral obligation to reduce externalities. This phenomenon resembles the results in the seminal paper by Gneezy and Rustichini (2000). They found that parents who were required to pay a new fine if they arrived late to pick up their children from day-care would arrive even later. A similar phenomenon could be present when paying for the marginal external costs. Individuals could feel to be entitled to emit more, as they have paid for their social damage. However, this argument does not explain why the same variance is seen for the information treatment as well. Further analysis could go into this and try to characterize this part of the treated population.

Limitations

There are some shortcomings of this master's thesis. Most importantly, the analysis of the average treatment effects could have focused more on the relative size of the effects. This was not developed as well, because of the limited size of this work and because the results of the quantile regressions did also allow to analyze this by showing which level of outcomes did react most. Another limitation in this thesis is the quality of the income variable. First of all, the income was assessed by a relatively vague question which did not specify which type of income the study asks about. In addition to that, no information on the composition of the households was available, which makes the construction of equivalized income variable very choice dependent. The robustness check in appendix I showed that the choice of the cutoff values for lower and higher income did not matter as much. However, this is a curious result and would require further investigation.

One concern within the MOBIS experiment is the topic of treatment salience. As described, treatment was received per email. Although the opening of the email could be tracked, there is no way of knowing if the treated individuals did actually read and understand it. Cleverly, the final survey of the MOBIS study included question which asked about the definition of external costs. Three options were given from which only one was correct. After four weeks of receiving information on the external costs of one's travel behavior one could expect individuals who understood the treatment, to answer this question correctly. However, only about half of all treated individuals answered correctly. Hence, that although problematic, this does only make the estimated treatment effects more conservative, as one could expect them to be higher if more individuals actually understood the treatment.

Another limitation of the MOBIS experiment is its questionable representativeness of the Swiss population and with that, the external validity of its result. The entry requirement for car travel led to about half of the initially contacted individuals not being allowed into the study. For this reason, one should take the results from this thesis with some caution, as the treatment effects might differ for individuals who do not travel by car as often. However, these individuals would also likely have lower external costs which was shown to be associated with lower treatment effects in the quantile regression analysis.

8 Conclusion

The goal of this thesis was to shed light on the treatment effect heterogeneity in the MOBIS experiment with respect to income. This is essential, as the Swiss government is currently discussing a law, which would provide a framework for the use of mobility pricing in practice. As with any tax, the implications for different income groups is of great policy interest.

The thesis started by introducing the economic background of the two treatments of the randomized control trial. Thereafter, the study setup and data set were presented. The important notion of equivalence income was also explained at this point, which allows this thesis to analyze the effect heterogeneity of income per person. This was the first extension to the final report of the MOBIS experiment by Axhausen et al. (2021). Overall, the construction of this variable did, however, not lead to new results, compared to the analysis with household income. No treatment effect could be measured for the nudge, on average. Charging the marginal external costs caused a significant reduction of total external costs by 0.23 CHF. This relates to a reduction of 5.1% for the overall treated population and was mostly driven by those individuals in the high income group (-0.37 CHF; -7.7%). However, the low income group also did show a 4.5% reduction relative to their original external costs.

Turning from average to quantile treatment effects, a u-shape was discovered for the pricing treatment. Both individuals with only few and those with substantial external costs did not respond significantly to the Pigovian tax. This leaves the average treatment effect to be mostly driven by individuals at the median of the external cost distribution. The fact, that no substantial treatment effect heterogeneity was found in absolute terms is good news for policy makers, as the effects seem to be quite homogeneous.

Future randomized control trials concerning mobility pricing or nudging could extend their analysis to include a higher share of individuals who do not travel by car twice per week. Also, it could be tried to combine a tracking study with location data from silicon valley tech giants (with the consent of the participants of course). Further research based on the MOBIS experiment could lay more focus on the relative effects of the treatments. Another interesting aspect would be to analyze the dynamics of the effects in more detail using an event study design.

Acknowledgments

I am very grateful for the valuable support and regular discussions with Prof. Dr. Beat Hintermann and Dr. Beaumont Schoemann who supervised this thesis. I also thank Lisa Triolo and Tobias Wagner for being wonderful study companions and the countless times they have helped me during the entire process. In addition to that, I want to thank Aljoscha Schöpfer for his valuable inputs and rigorous corrections in the final stage.

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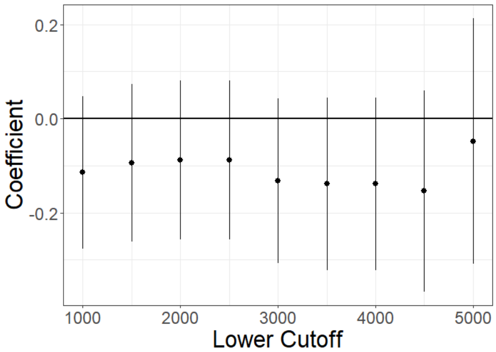
I Appendix

I Robustness Check for Income Groups

Figure I: Robustness check for low income group cutoff



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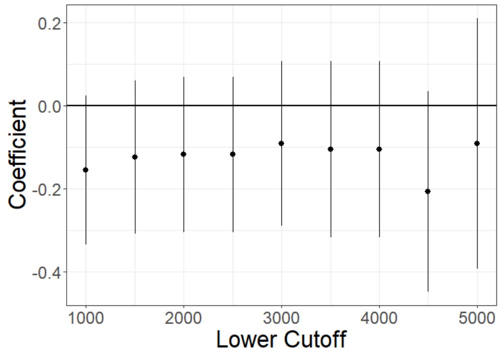
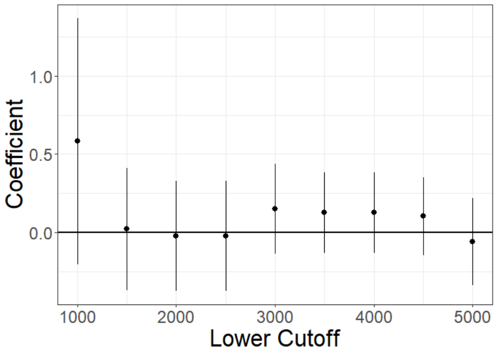
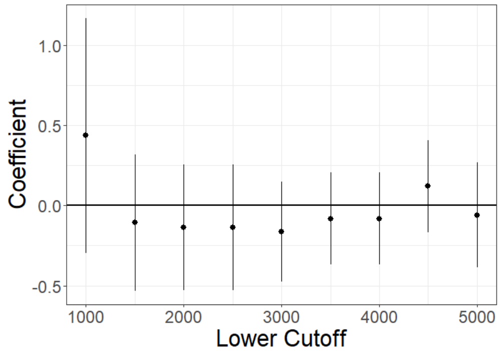


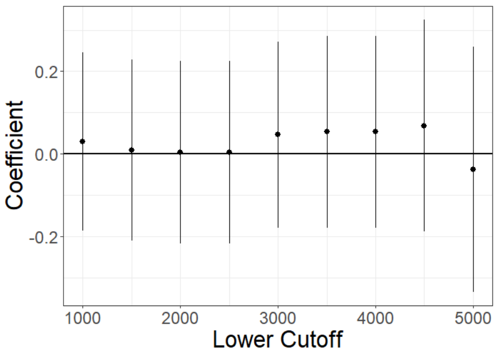
Pricing



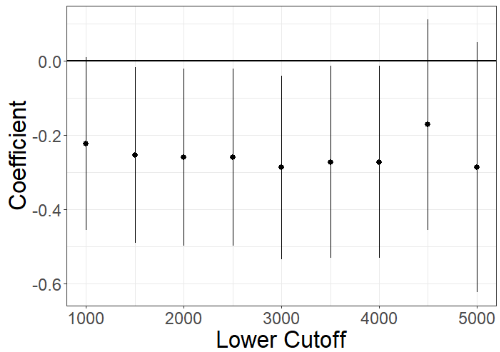
Low income x info



Low income x pricing



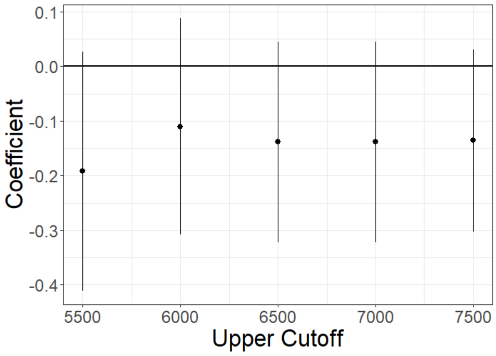
High income x info



High income x pricing

Notes : Robustness check for the choice of 4,000 CHF as the cutoff for the low income group. Fixed effects regression with income group interactions is performed for the total external cost variable. Cutoff defining low income group is varied from < 1,000 CHF up to < 5,000 CHF. Cutoff for high income group is held constant at 6,500 CHF.

Figure II: Robustness check for high income group cutoff



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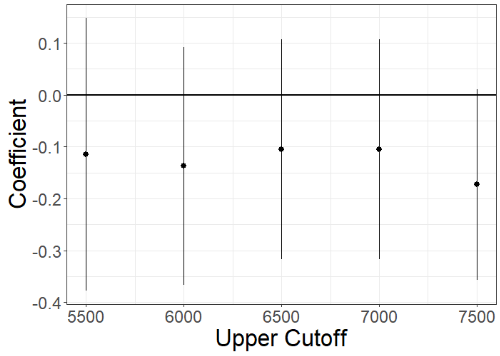

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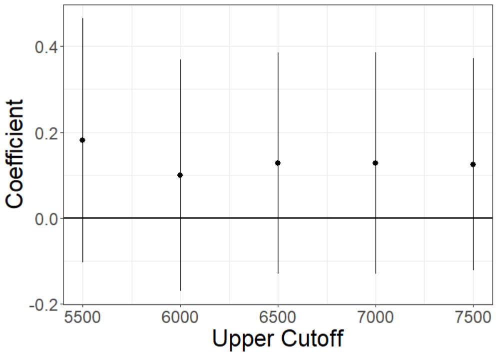

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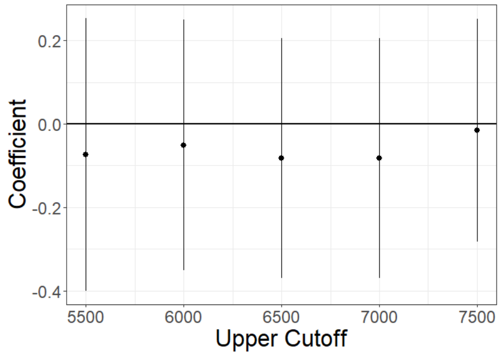
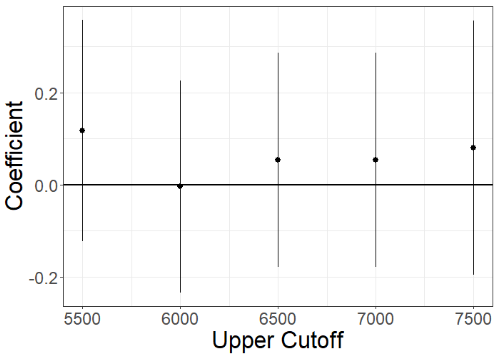
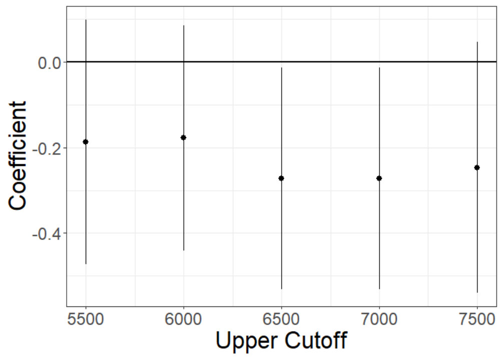


Low income x pricing



High income x info



High income x pricing

Notes : Robustness check for the choice of 6,500 CHF as the cutoff for the high income group. Fixed effects regression with income group interactions is performed for the total external cost variable. Cutoff defining high income group is varied from > 5,500 CHF up to > 7,500 CHF. Cutoff for low income group is held constant at 4,000 CHF.

II QTE Estimation Tables per Income Group

Table I: Output table QTET conditional on low income

	Information				Pricing			
τ	Total	Congestion	Climate	Health	Total	Congestion	Climate	Health
0.05	0.05	-0.06*	-0.01	0.03	0.15	-0.03	0.01	0.18*
	(0.16)	(0.03)	(0.03)	(0.10)	(0.17)	(0.03)	(0.03)	(0.10)
0.10	-0.10	-0.06*	0.02	-0.02	-0.05	-0.03	-0.01	0.02
	(0.16)	(0.03)	(0.04)	(0.09)	(0.18)	(0.03)	(0.03)	(0.10)
0.15	-0.07	-0.07*	0.01	-0.05	-0.02	-0.05	-0.01	-0.06
	(0.17)	(0.04)	(0.03)	(0.10)	(0.16)	(0.04)	(0.04)	(0.11)
0.20	-0.19	-0.04	-0.00	-0.07	-0.17	-0.05	-0.04	-0.11
	(0.18)	(0.04)	(0.03)	(0.13)	(0.19)	(0.03)	(0.03)	(0.14)
0.25	-0.22	-0.02	-0.00	-0.18	-0.32	-0.04	-0.06	-0.26**
	(0.21)	(0.04)	(0.04)	(0.13)	(0.23)	(0.04)	(0.04)	(0.13)
0.30	-0.30	-0.02	-0.02	-0.22*	-0.58***	-0.03	-0.10*	-0.27**
	(0.24)	(0.04)	(0.04)	(0.12)	(0.22)	(0.04)	(0.04)	(0.12)
0.35	-0.32*	-0.01	-0.02	-0.20*	-0.58***	-0.01	-0.08**	-0.25**
	(0.18)	(0.04)	(0.04)	(0.12)	(0.18)	(0.05)	(0.04)	(0.11)
0.40	-0.41**	0.03	-0.06	-0.23*	-0.63***	-0.01	-0.12***	-0.30**
	(0.17)	(0.05)	(0.04)	(0.13)	(0.20)	(0.05)	(0.05)	(0.13)
0.45	-0.33*	0.03	-0.08*	-0.10	-0.45**	-0.01	-0.09*	-0.34**
	(0.18)	(0.05)	(0.04)	(0.14)	(0.22)	(0.05)	(0.05)	(0.13)
0.50	-0.32*	0.05	-0.14***	-0.16	-0.47**	-0.01	-0.14***	-0.29**
	(0.18)	(0.05)	(0.05)	(0.12)	(0.20)	(0.05)	(0.05)	(0.13)
0.55	-0.44***	0.04	-0.13***	-0.14	-0.39*	-0.04	-0.11*	-0.23*
	(0.16)	(0.06)	(0.05)	(0.12)	(0.21)	(0.06)	(0.06)	(0.13)
0.60	-0.39*	0.01	-0.15***	-0.11	-0.37	-0.04	-0.10*	-0.22
	(0.20)	(0.06)	(0.05)	(0.13)	(0.25)	(0.06)	(0.06)	(0.14)
0.65	-0.21	0.04	-0.12**	-0.20	-0.41	-0.01	-0.06	-0.21
	(0.25)	(0.07)	(0.06)	(0.16)	(0.29)	(0.07)	(0.06)	(0.18)
0.70	-0.41	0.00	-0.07**	-0.40	-0.53	-0.07	-0.06	-0.45**
	(0.31)	(0.08)	(0.06)	(0.19)	(0.32)	(0.08)	(0.06)	(0.19)
0.75	-0.35	-0.06	-0.02*	-0.41	-0.50	-0.07	-0.03	-0.36*
	(0.29)	(0.09)	(0.06)	(0.24)	(0.30)	(0.09)	(0.06)	(0.21)
0.80	-0.07	-0.01	-0.00	-0.05	-0.34	0.02	-0.05	-0.32
	(0.41)	(0.09)	(0.07)	(0.24)	(0.32)	(0.10)	(0.07)	(0.20)
0.85	0.51	0.02	0.01	0.21	-0.18	-0.04	-0.05	-0.14
	(0.36)	(0.13)	(0.11)	(0.27)	(0.35)	(0.15)	(0.08)	(0.21)
0.90	0.69	-0.07	0.11	0.53	-0.01	-0.06	-0.16	-0.14
	(0.57)	(0.18)	(0.14)	(0.29)	(0.49)	(0.17)	(0.12)	(0.28)
0.95	1.48	0.14	0.32	0.75	0.78	0.05	0.15	0.03
	(0.94)	(0.20)	(0.18)	(0.51)	(0.82)	(0.27)	(0.19)	(0.54)
ATET	-0.06	-0.00	-0.01	-0.05	-0.22	-0.01	-0.05	-0.16
	(0.15)	(0.05)	(0.04)	(0.10)	(0.16)	(0.05)	(0.04)	(0.10

Notes: Quantile regression output for entire sample. Standard errors (in parentheses) calculated with 1,000 bootstrap iterations.

Table II: Output table QTET conditional on reference income

	Information				Pricing			
τ	Total	Congestion	Climate	Health	Total	Congestion	Climate	Health
0.05	-0.04	-0.02	-0.01	-0.01	-0.08	-0.02	-0.01	0.02
	(0.15)	(0.03)	(0.03)	(0.09)	(0.16)	(0.03)	(0.03)	(0.09)
0.10	-0.10	0.00	-0.04	-0.10	0.00	-0.02	-0.03	-0.10
	(0.14)	(0.03)	(0.03)	(0.09)	(0.16)	(0.03)	(0.03)	(0.09)
0.15	-0.12	-0.03	-0.03	-0.07	-0.04	-0.02	-0.05*	0.01
	(0.15)	(0.03)	(0.03)	(0.08)	(0.16)	(0.03)	(0.03)	(0.09)
0.20	-0.12	-0.01	-0.02	-0.00	0.05	-0.01	-0.00	0.07
	(0.13)	(0.03)	(0.03)	(0.08)	(0.17)	(0.03)	(0.03)	(0.09)
0.25	0.02	-0.02	-0.00	-0.00	0.14	-0.01	0.01	0.04
	(0.15)	(0.03)	(0.04)	(0.08)	(0.14)	(0.03)	(0.03)	(0.08)
0.30	-0.02	-0.00	0.05*	0.06	0.10	-0.01	0.03	0.07
	(0.14)	(0.04)	(0.03)	(0.08)	(0.15)	(0.03)	(0.03)	(0.09)
0.35	0.04	0.01	0.02	0.09	0.14	-0.01	0.02	0.10
	(0.13)	(0.03)	(0.03)	(0.09)	(0.14)	(0.04)	(0.03)	(0.09)
0.40	-0.03	-0.00	0.02	0.03	0.11	-0.01	0.03	0.06
	(0.15)	(0.04)	(0.03)	(0.09)	(0.15)	(0.04)	(0.03)	(0.10)
0.45	-0.11	-0.03	0.02	0.02	0.04	-0.03	0.04	0.07
	(0.17)	(0.04)	(0.03)	(0.10)	(0.16)	(0.04)	(0.03)	(0.11)
0.50	-0.11	-0.04	0.01	-0.05	-0.06	-0.05	0.04	0.06
	(0.18)	(0.04)	(0.04)	(0.11)	(0.17)	(0.04)	(0.04)	(0.12)
0.55	-0.05	-0.04	0.00	-0.13	-0.09	-0.07	0.04	-0.07
	(0.20)	(0.05)	(0.04)	(0.12)	(0.19)	(0.05)	(0.04)	(0.12)
0.60	0.09	-0.05	0.01	-0.02	-0.08	-0.08	0.04	-0.10
	(0.18)	(0.05)	(0.04)	(0.11)	(0.19)	(0.05)	(0.04)	(0.11)
0.65	-0.02	-0.07	-0.00	-0.06	-0.29	-0.07	0.01	-0.23
	(0.18)	(0.05)	(0.04)	(0.12)	(0.21)	(0.05)	(0.04)	(0.14)
0.70	-0.32	-0.08	-0.03	-0.09	-0.36	-0.07	-0.04	-0.14
	(0.20)	(0.06)	(0.05)	(0.11)	(0.23)	(0.06)	(0.04)	(0.15)
0.75	-0.36	-0.10	-0.06	-0.07	-0.62**	-0.10	-0.10*	-0.08
	(0.26)	(0.07)	(0.06)	(0.14)	(0.25)	(0.07)	(0.06)	(0.14)
0.80	-0.32	-0.16**	-0.08	-0.13	-0.64**	-0.11	-0.12*	-0.16
	(0.22)	(0.07)	(0.06)	(0.14)	(0.26)	(0.08)	(0.07)	(0.16)
0.85	-0.32	-0.03	-0.09	-0.14	-0.42	-0.14	-0.13*	-0.12
	(0.30)	(0.09)	(0.07)	(0.19)	(0.37)	(0.11)	(0.08)	(0.21)
0.90	-0.12	-0.09	-0.03	-0.22	0.01	-0.07	-0.05	-0.08
	(0.40)	(0.11)	(0.07)	(0.27)	(0.43)	(0.13)	(0.08)	(0.27)
0.95	-0.74	-0.11	-0.08	-0.24	0.14	-0.19	0.05	-0.06
	(0.56)	(0.21)	(0.12)	(0.37)	(0.86)	(0.25)	(0.16)	(0.44)
ATET	-0.15	-0.05	-0.02	-0.08	-0.05	-0.06	0.01	-0.00
	(0.12)	(0.04)	(0.03)	(0.07)	(0.14)	(0.04)	(0.03)	(0.09)

Notes: Quantile regression output for entire sample. Standard errors (in parentheses) calculated with 1,000 bootstrap iterations.

Table III: Output table QTET conditional on high income

	Information				Pricing			
τ	Total	Congestion	Climate	Health	Total	Congestion	Climate	Health
0.05	-0.31*	-0.07*	-0.05	-0.09	-0.21	-0.09**	-0.05	-0.08
	(0.17)	(0.04)	(0.04)	(0.09)	(0.17)	(0.04)	(0.04)	(0.09)
0.10	-0.21	-0.03	-0.06*	-0.09	-0.13	-0.09**	-0.03	0.03
	(0.15)	(0.04)	(0.03)	(0.10)	(0.15)	(0.04)	(0.03)	(0.08)
0.15	-0.27	-0.01	-0.05	-0.06	-0.12	-0.09**	-0.02	0.03
	(0.17)	(0.04)	(0.04)	(0.10)	(0.18)	(0.04)	(0.03)	(0.11)
0.20	-0.27*	-0.04	-0.05*	-0.24**	-0.20	-0.09**	-0.03	-0.13
	(0.15)	(0.04)	(0.03)	(0.11)	(0.14)	(0.04)	(0.03)	(0.10)
0.25	-0.25*	-0.02	-0.04	-0.18**	-0.28*	-0.10**	-0.02	-0.15
	(0.15)	(0.04)	(0.03)	(0.09)	(0.16)	(0.04)	(0.03)	(0.10)
0.30	-0.20	-0.04	-0.03	-0.19*	-0.31*	-0.09**	-0.06*	-0.19*
	(0.15)	(0.04)	(0.04)	(0.11)	(0.17)	(0.04)	(0.03)	(0.10)
0.35	-0.31*	-0.05	-0.03	-0.19*	-0.35**	-0.11**	-0.06	-0.25***
	(0.19)	(0.04)	(0.03)	(0.10)	(0.18)	(0.04)	(0.04)	(0.09)
0.40	-0.23	-0.03	-0.04	-0.24**	-0.38***	-0.12**	-0.09	-0.26***
	(0.19)	(0.05)	(0.04)	(0.10)	(0.17)	(0.05)	(0.04)	(0.09)
0.45	-0.17	0.01	-0.07	-0.07	-0.53***	-0.10**	-0.09**	-0.29***
	(0.18)	(0.05)	(0.04)	(0.13)	(0.18)	(0.05)	(0.04)	(0.10)
0.50	-0.12	-0.03	-0.07	-0.04	-0.49***	-0.08	-0.09**	-0.25**
	(0.17)	(0.05)	(0.05)	(0.11)	(0.18)	(0.05)	(0.04)	(0.12)
0.55	-0.06	-0.00	-0.05	-0.07	-0.29	-0.11**	-0.06	-0.32**
	(0.17)	(0.05)	(0.04)	(0.13)	(0.20)	(0.05)	(0.04)	(0.13)
0.60	-0.04	-0.01	-0.06	-0.00	-0.27	-0.08	-0.06	-0.22*
	(0.18)	(0.06)	(0.04)	(0.12)	(0.19)	(0.06)	(0.04)	(0.13)
0.65	-0.00	0.03	-0.08*	0.02	-0.32	-0.11*	-0.05	-0.21
	(0.20)	(0.06)	(0.04)	(0.12)	(0.21)	(0.07)	(0.05)	(0.13)
0.70	0.02	-0.01	-0.05	0.02	-0.26	-0.10	-0.02	-0.10
	(0.23)	(0.06)	(0.05)	(0.14)	(0.26)	(0.07)	(0.05)	(0.16)
0.75	-0.06	-0.05	-0.02	-0.12	-0.24	-0.12	-0.07	-0.09
	(0.32)	(0.07)	(0.06)	(0.16)	(0.31)	(0.07)	(0.06)	(0.20)
0.80	-0.01	-0.06	-0.01	-0.11	-0.43	-0.24**	-0.09	0.01
	(0.32)	(0.10)	(0.07)	(0.22)	(0.32)	(0.11)	(0.07)	(0.18
0.85	-0.15	-0.09	0.04	0.12	-0.56**	-0.19*	-0.03	-0.17
	(0.31)	(0.11)	(0.08)	(0.25)	(0.28)	(0.10)	(0.08)	(0.20)
0.90	0.32	0.01	0.02	0.07	-0.58	-0.16	-0.00	-0.28
	(0.48)	(0.13)	(0.12)	(0.32)	(0.41)	(0.12)	(0.12)	(0.24)
0.95	0.27	-0.05	-0.02	0.18	-0.32	-0.09	-0.01	-0.31
	(0.59)	(0.21)	(0.14)	(0.38)	(0.55)	(0.18)	(0.16)	(0.37)
ATET	-0.08	-0.01	-0.04	-0.04	-0.33**	-0.11***	-0.05*	-0.16**
	(0.13)	(0.04)	(0.03)	(0.08)	(0.13)	(0.04)	(0.03)	(0.08)

Notes: Quantile regression output for entire sample. Standard errors (in parentheses) calculated with 1,000 bootstrap iterations.